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EXPERIMENTAL RESEARCHES

APPLIED TO

PHYSIOLOGY AND PATHOLOGY.

BY

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THE papers collected in this book have been published in the Medical Examiner, of Philadelphia, from August, 1852, to August, 1853. They form only a part of the author's original researches in Physiology and Pathology. A second will soon be published, containing the results of the author's experiments and clinical observations on some important points of the physiology and pathology of the different nervous centres, on fractures of the spine, on the vital properties of the iris and of the cellular tissue, on the properties and functions of the blood, on the signs of death, on electro-physiology, on the laws of cadaveric rigidity and putrefaction, and on the etiology and treatment of some of the nervous diseases.



ERRATA.

Page 28, line 31; is at, *read* is not at.

" 54, " 15; animation, *read* diminution.

" 55, 1st noto; Kay, *read* Fray.

Pages 58 & 75, A. Barnard, *read* Cl Bernard.

Page 73, 2d note; 1853, *read* 1852; and add or, *unto* Art. III., p. 9.

" 78, line 35; molar, *read* motor.

" " 38; contraction of or, *read* contraction or.

" 103, " 13; *after* muscles, *add* of this last limb.

" 116, " 16; to account, *read* easy to account.

" 117, " 15 of the note; any, *read* the.

" " 2d noto; Bologne, *read* Biologic.

" 119, line 6; Does that, *read* Why does.

" " 26; minutes, *read* times.

" " 30; any, *read* a.

" 123, " 37; so, *read* very.



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[Reprinted from the Medical Examiner for August, 1852.]

I.—ON THE SOURCE OF THE VITAL PROPERTIES.

I think that every tissue possesses its vital properties, in consequence of its peculiar organization, and that in a completely developed animal, nutrition is the source of the vital properties, inasmuch as it is the cause of the maintenance of organization.

I will try to prove the correctness of my opinion, by the following remarks on some of the vital properties of the spinal cord, the nerves, and the muscles.

[* The paper which we have the pleasure of presenting to our readers from Dr. Brown-Séquard, is a resumé of many researches made by the author, a part of which only have hitherto been published in any of the foreign journals.

The conclusions arrived at are the result of eight years exclusive devotion to the experiments upon which they are based.—Eds. Ex.]

a.—*Source of the reflex faculty in the spinal cord.*

Notwithstanding the experiments of Redi, Whytt, Prochaska, Unzer, Sénac, Fontana, Caldani, Sir G. Blane, Fray, Legallois and many other experimenters; and notwithstanding the much more important experiments of Marshall Hall, Müller, Grainger, Volkmann, Kürschner, Pickford, de Martino, Buchner, Mayer, Paton and Stilling, the existence of the reflex faculty, after the spinal cord has been separated from the encephalon, is not considered by all physiologists as a proof of the independence of the spinal cord. J. W. Arnold and Flourens still maintain that the medulla oblongata is a centre, giving life to the other parts of the nervous system. The reflex faculty possessed by the spinal cord after it has been separated from the encephalon, is considered by J. W. Arnold as a remainder of something given to the spinal marrow by the encephalon, before their separation.

My experiments prove the incorrectness of that opinion.* I have found that after having exhausted the reflex faculty by putting it in action, energetically and frequently, in an animal on whom the spinal cord is separated from the encephalon, it reappears, and becomes soon as energetic as before, provided that the circulation of blood takes place in the cord. Moreover I have found, that if the reflex faculty is put in action frequently, it is able to produce an immense quantity of action: thus, for instance, it can stimulate sufficiently the muscles of a frog's leg to make them raise, in an hour and in divided portions, about twelve pounds, to the height of about two lines. In a pigeon the reflex faculty is able to stimulate the muscles of a leg so far as to make them raise fifty pounds, by fractions, in an hour, to the height of more than one inch.†

I shall add two other decisive proofs:—1. The reflex faculty is very weak in frogs immediately after the spinal cord has been separated from the medulla oblongata, and it increases afterwards, as R. Whytt and Marshall Hall have discovered. I have stated that it increases so much that the posterior limbs are able

* See:—*Recherches et expériences sur la physiologie de la moelle épinière*. Thèse inaugurale. Paris, 3 Janvier, 1846.—*Comptes rendus des séances de l'Académie des Sciences*. Paris, 1847 T. xxiv. p. 849.

† See *Gaz. Méd. de Paris*. T. 4. 1849. p. 233.

to draw up, by reflex action, more than double the weight the animal could raise up by an action of its will before the division of the cord. 2. After having divided the spinal cord in the dorsal region on a mammal, I kill it by cutting the right carotid artery. A few minutes after the cessation of reflex action I inject blood by the opening made in the carotid. Then life returns and with it the reflex faculty.

All these facts demonstrate positively that the reflex faculty is a vital property belonging to the spinal cord, and that its source is in the nutrition which maintains the organization of that nervous centre.

b.—*Source of the vital property of the motor nerves.*

The independence of the motor nerves is denied by almost all physiologists. They believe that the nervous centres are the sources of the vital property of these nerves. They base their opinion on this fact, that the motor nerves separated from the nervous centres soon lose their property, as it has been seen by Fontana, Haighton, Astley Cooper, Steinrueck, Müller, Stieker, Günther, Schoen, Kilian, Stannius, Helmholtz, Martin-Magron and others.

But, *in the first place*, if the motor nerves of the warm-blooded animals lose their vital property after having been separated from the nervous centres, it is not less positive that they retain it during several days. *Secondly*, if the vital property of the motor nerves is exhausted by very energetic action, it reappears after a short time, although the nerves are separated from the cerebro-rachidian centre, provided that the circulation of blood continues in them. *Thirdly*, if the circulation of blood is stopped in a limb in which the nerves have been divided, it is found that the peripheric portion of the divided nerves lose their vital property before the muscles. After the nerves have been left dead, *i. e.*, deprived of their vital property for a quarter of an hour, half an hour, and even more, blood is allowed to circulate anew in the limb. Then the vital property of the cut nerves returns, and, to produce a muscular contraction, only a slight compression upon them is necessary.* If the

* See Comptes rendus de l'Acad. des Sciences. T. xxxii. Séance du 9 Juin, 1851.—Gaz. Médic. de Paris. 1851. T. vi, p. 359.

motor nerves lose their property when they are separated from the nervous centres, it is because they are then badly nourished. Nerves as well as muscles must be exercised, in order to be well nourished.

c.—*Source of the muscular contractility.*

Although there are some facts which appear strongly to prove that the vital property of the muscular tissue is independent of the nervous system, many physiologists persist in their opposition to Haller's doctrine on this subject. Therefore I have thought necessary to add new proofs to those already known, and I have published many experiments, of which I shall relate here only two of the most decisive.*

1. The sciatic and the crural nerves having been resected, for ten or twelve days, on a rabbit or a guinea-pig, I examine if these nerves have completely lost their vital property, and if the muscles are still contractile. When this has been ascertained, I put a ligature around the aorta. Then muscular irritability disappears after a certain time and cadaveric rigidity appears. Three quarters of an hour or even an hour after the complete disappearance of the muscular irritability, and the appearance of the *rigor mortis*, I cut off the ligature, and I find, after ten or fifteen minutes, that the rigidity disappears and the contractility reappears. I need not say that the nerves do not regain their lost property. This fact clearly proves that the contractility is given to the muscles by blood, *i. e.*, by nutrition, and not by the nervous system.

2. Many experiments have shown to me that muscles paralyzed for five days or a little more, in consequence of the division of their nerves, remain much longer contractile after the death of the animal than the non-palsied muscles. This would hardly be the case if the contractility was given to muscles by the nervous system.

* See:—Bulletin de la Soc. Philomat. 1847, p. 74.—Gaz. Méd. de Paris, 1851, t. vi. p. 619, and 1852, t. vii. p. 72.

II.—RESEARCHES ON THE REFLEX FACULTY.

During the last seven years I have published many papers relating to the reflex faculty.* Among the facts which I have discovered I will mention the following :

1. Grainger had found that the act of suckling can be executed by an animal deprived of its brain. I have found that even after the ablation of both the brain and the cerebellum, newly-born rabbits are able to suckle very well; which is a proof that suckling may be executed by reflex action.

2. It is commonly affirmed that the reflex power is much stronger in cold-blooded than in warm-blooded animals. This opinion is correct so far as regards the contrast between Mammals and Batrachia (the animals usually compared); but it is incorrect if Birds are compared with Reptilia and Fishes. It has been said that the higher an animal is in the scale the less it has reflex power. If this be true, we should find more and more reflex power from Mammals to Fishes; but the real order, according to my experience is : 1st, Fishes; 2d, Mammals; 3d, Amphibia and Reptilia; 4th, Birds; so that Birds have more reflex power than all the other animals, and Mammals have more than Fishes. Of course, there are exceptions to this rule in the case of particular species; thus the eel, carp and tench have as much reflex power as many Mammals possess.

It has also been commonly affirmed that the reflex power diminishes with age, being the greatest in young animals. This statement, also, has been based on a too limited induction. In Reptiles and Fishes no difference can be detected in this particular. In Birds it is decidedly the other way, the reflex power being much the strongest in adults. Among Mammals the difference is usually in favor of the young animal; not, however, at the very earliest part of its life, but ten or twelve days after birth. As to man the reflex power appears to be greater in him than in Fishes and Mammals; but it is not so energetic as in Birds and in Amphibia.

I have found that the causes of the differences between differ-

* See my Inaugural Dissertation, Paris, 3 Janvier 1846, 1^{re} partie.—Comptes rendus de l'Acad. des Sciences, 1847, t. xxiv. pp. 363 et 859.—Gaz. Med. de Paris, 1849, t. iv. pp. 430 et 644; et 1850 t. v. pp. 98 et 476.

ent animals, as regards the energy of their reflex power, are to be explained by anatomical differences. There exists a constant relation between the degree of the reflex power and the amount of grey matter in the spinal cord. It appears, also, that the mode of circulation of the blood in the spinal marrow has a great share in the causes of differences amongst different animals.

3. It is not necessary for the existence of the reflex power that the spinal cord should be without alteration. I have found the reflex faculty remaining in pigeons after I had crushed the spinal cord, and produced in it a considerable alteration. This is important to be known by practitioners, to prevent their drawing the conclusion, from the existence of reflex action after a fracture or a luxation of the vertebral column in man, that the spinal cord is healthy.

4. The influence of the nervous system on the secretions, by a reflex action, has been very little studied. I will state two examples of these reflex secretions: 1st, There is on the face, and particularly on the forehead and the nose, an abundant production of sweat when the nerves of the taste are strongly excited, as they are, for instance, by common salt, pepper, sugar, etc. In certain persons the quantity of sweat produced in such cases is sometimes, even in the winter, very considerable. 2d, I have observed that it is sufficient to excite the nerves of taste in order to produce the secretion of gastric juice, bile and pancreatic juice.

III.—RESEARCHES ON THE INFLUENCE OF THE NERVOUS SYSTEM UPON THE FUNCTIONS OF ORGANIC LIFE.

My experiments have convinced me that if it is certain that the nervous system is able to act, and frequently does act, on the functions of organic life, it is not the less certain that the action of the nervous system on these functions is not necessary. I hope this will be sufficiently demonstrated by the numerous facts I have to relate.

a. Influence of the section of nerves on nutrition and secretion.

1. The frequent occurrence of certain pathological changes after section of the sciatic nerve in Mammals, has been cited as

a proof of the dependence of the nutritive operations upon nervous agency. I think the following experiments give evidence against that doctrine. I have divided the sciatic nerve in a number of rabbits and guinea-pigs, and placed some of them at liberty in a room with a paved floor, whilst I confined others in a box, the bottom of which was thickly covered with bran, hay and old clothes. In a fortnight, the former set exhibited an obviously disordered action in the paralysed limbs; the claws were entirely lost; the extremities of the feet were swollen, and the exposed tissues were red, engorged, and covered with fleshy granulations. At the end of a month, these alterations were more decided, and necrosis had supervened in the denuded bones. On the other hand in the animals confined in the boxes, no such injuries had accrued; and although some of them have been kept living for four, five and even six months after the division of the sciatic nerve, no alteration whatever has appeared in the palsied limbs except atrophy. In these cases a portion of the nerve had been cut off, so that reunion was nearly impossible and did not take place.

Experiments made on pigeons have given the same results.

It is obvious from these experiments that the pathological changes which occur after the section of the sciatic nerve do not proceed directly from the absence of nervous action, but that they are consequent upon the friction and continual compression to which the paralysed limbs are subject, against a hard soil, owing to the inability of the animal to feel or avoid it.

In similar experiments made on frogs, I found that no alteration took place, except when water penetrated through the wound, under the skin, and between the muscles.*

2. With the help of an eminent micrographer (Dr. Lebert), I have made researches on the influences produced on the capillary circulation in consequence of the section of all the nerves (sympathetic and cerebro-spinal nerves) in the legs of a number of frogs. We have found no appearance of trouble in the capillary circulation, neither in an hour, nor in three or four days after the division of the nerves.

3. When resection of a long portion of one of the sciatic and the crural nerves is made on a very young rabbit, guinea-

* See Gaz. Méd. de Paris, 1849, t. 4, p. 880.

pig or pigeon, the palsied limb continues to grow in length, but it grows only very little, if at all, in thickness. When the experiment is made on all the nerves of the wing in a very young pigeon, it is also found that the wing grows in length, but very little in breadth or in thickness. The secretion of quills takes place equally as well in the palsied limb as in the other.

The difference in all these cases between the length of the sound and that of the palsied limb or wing is never very considerable; nevertheless the length of the healthy parts is greater than that of the paralysed parts.

4. I have found that burns, wounds and ulcerations existing in parts palsied in consequence of the section of their cerebro-spinal nerves, are cured as quickly and as well as those in sound parts.

5. Atrophy is a constant consequence of the section of the nerves of a limb. I have found that it supervenes not only in the muscles and the bones, as J. Reid has discovered, but also in the skin, which becomes evidently thinner.

6. Krimer asserts that after the section of the nerves of a limb in Mammals, the venous blood is of a bright red color like the arterial blood. (*Physiologische Untersuchungen*, Leipzig, 1820, p. 138 exp. 1, and p. 152 exp. 9.)

Long before the publication of Krimer, Arnemann had declared that the blood appeared darker than usual in a limb on which all the nerves had been cut. (*Versuche über die Regeneration an lebenden thieren*, Gottingen, 1786, t. i., p. 48.)

Longet (*Traité de Physiologie*, Paris, 1850, t. ii., B. p. 92,) says that he has seen the venous blood retaining its ordinary color even three days after the section of the nerves of the anterior limb in dogs.

Who is right—Krimer, Arnemann or Longet? Neither of them is perfectly right. The assertion of Arnemann is entirely incorrect. By experiments made on dogs, rabbits, guinea-pigs and pigeons, I have found that the venous blood in palsied limbs is evidently less black than it is in sound limbs. But it is not true to say that venous and arterial blood in paralysed limbs have the same color. It is always very easy to distinguish one from the other.

The transformation of the arterial blood into venous is not so

complete in the palsied as in the sound limb, but it always takes place even in a great measure. There is a good proof of this in the result of my experiments on the hand and forearms of two decapitated men. I injected blood in the arteries of these parts thirteen or fourteen hours after death and when cadaveric rigidity existed. Surely there was in that case no nervous action whatever, and nevertheless the blood, which was of a bright red color when injected, came out nearly black from the veins!

From all these facts I shall conclude:

1st, That the nervous action (that of the sympathetic as well as that of the cerebro-spinal nerves) is not necessary for the change of color of the blood in the capillaries.

2d, That the nervous system of animal life has an influence upon nutrition by which it takes a share in the transformation of arterial into venous blood.

7. My friend Dr. Cl. Bernard has recently discovered the curious fact, that after the section of the sympathetic nerve in the neck, the face on the same side and more particularly the ear, become warmer and more sensible than the other side. The blood-vessels are much enlarged and a great many are visible which were not so before the operation.

I have found that the remarkable phenomena which follow the section of the cervical part of the sympathetic, are mere consequences of the paralysis and therefore of the dilatation of the bloodvessels. The blood finding a larger way than usual, arrives there in greater quantity; therefore the nutrition is more active. Now the sensibility is increased because the vital properties of the nerves are augmented when their nutrition is augmented. As to the elevation of the temperature, I have seen, as Dr. Bernard has, that the ear exhibits, sometimes, one or two degrees Fahr. more than the rectum; but it must be remarked that the temperature of the rectum is a little lower than that of the blood; and as the ear is full of blood, it is very easy to understand why it has the temperature of the blood. A great many facts prove that the degree of temperature and of sensibility of a part, is in close relation with the quantity of blood circulating in that part.

I base my opinion in part on the following experiments: If galvanism is applied to the superior portion of the sympathetic

after it has been cut in the neck, the vessels of the face and of the ear after a certain time begin to contract; their contraction increases slowly, but at last it is evident that they resume their normal condition, if they are not even smaller. Then the temperature and the sensibility diminish in the face and the ear, and they become in the palsied side the same as in the sound side.

When the galvanic current ceases to act, the vessels begin to dilate again, and all the phenomena discovered by Dr. Bernard reappear.

I conclude, that the only direct effect of the section of the cervical part of the sympathetic, is the paralysis and consequently the dilatation of the bloodvessels. Another evident conclusion is, that the cervical sympathetic send motor nerve fibres to many of the bloodvessels of the head.*

8. Nearly all physiologists believe that the secretion of the gastric juice is stopped after the section of the two pneumogastric nerves. It is difficult to solve the question by experiments on warm-blooded animals, because they die too quickly after the section of the vagi. But it is not so with frogs. I have found that they are able to live perfectly well either after the extirpation of the medulla oblongata, or after the extirpation of the ganglia of the par vagum. In both these cases I have found that digestion continues to be performed. Consequently, if the gastric juice is necessary to digestion, it is certain that this liquid is secreted.†

* My experiments prove, also, that the bloodvessels are contractile, and that the nerves are able to put them in action. I have also to remark that it is a fact, well established by Budge and Waller, that the cervical sympathetic is one of the motor nerves of the iris, and that the spinal cord is the origin of the nerve-fibres going from the sympathetic to the iris. Some experiments, which I intend to perform again, appear to prove that the same parts of the spinal cord which give origin to some of the motor nerve-fibres of the iris, originate also the motor nerve-fibres going from the cervical sympathetic to the vessels of the head. Another conclusion is to be drawn from the results obtained by Budge, Waller, Bernard and myself; it is that the cervical sympathetic, instead of receiving its fibres from upwards to give them downwards, receives them downwards and distributes them upwards.

† Comptes rendus de l'Acad. des Sciences. Paris, 1847. T. xxiv. p. 363-64.

9. J. Reid has found, that if the four nerves uniting the spinal cord to the posterior limbs are cut across on both sides, in frogs, and if a galvanic current is applied every day to the palsied limbs on one side, these galvanized limbs retain their natural dimensions, while the palsied limbs not galvanized become atrophied.

I have found:—1. That if, instead of cutting only the four cerebro-spinal nerves of the posterior limbs, I divide also the branches of the sympathetic nerve which unite with them, the same results are obtained as in Reid's experiment. 2. That if a like experiment is performed on dogs, guinea-pigs, rabbits and pigeons, the same results are found. 3. That if after atrophy has taken place in the limb of a mammal or a pigeon, a galvanic current is applied, every day, during several weeks, the atrophy diminishes little by little and the limb at length becomes as large as a sound limb. This happens although there is no return of vital property in the divided nerves. 4. That if the application of galvanism is made on the palsied limbs of very young animals, and continued every day until they have arrived at adult age, these limbs are then found to have grown as much, in every respect, as the sound limbs.

In addition to these facts I have to state that in cases of lead palsy, in which the extensor muscles, as far as I have been able to judge, were completely destroyed and replaced by fibrous tissue, I have seen muscles created by galvanism and becoming as strong as they are in healthy men.

In a case, which I published two years ago, (*Gaz. Méd. de Paris*, 1850, t. v. p. 169,) I have found that an increase of five centimetres in circumference took place in the superior part of the leg of a young gentleman, under the influence of galvanism, applied three quarters of an hour each day for six days.

In all the facts before related, galvanism acts by two ways: the one is that it exercises the muscles, and increase in consequence their nutrition; the other is that it produces directly some of the chemical changes which constitute nutrition.

The atrophy, which happens in paralyzed muscles, takes place mostly because they remain without exercise, and partly because when nervous action is deficient the respiration of the muscles is not carried on as well as when the nervous system acts upon

them. Galvanism applied to a palsied limb acts partly in producing the transformation of arterial into venous blood, *i. e.*, what Gustav Liebig calls the respiration of the muscles. I have seen frequently the venous blood, in palsied limbs, becoming as black as normal venous blood, after the application of galvanism. This change of coloration is not produced by a direct chemical influence, exerted by galvanism on the blood, for if galvanism is applied to blood in a vase, nothing of that kind is seen. It is in consequence of an interchange between blood and the living tissues that the change of color happens. The muscular contraction which takes place under the influence of the nervous system, or that of galvanism, produces, in both cases, an increase in the darkness of the venous blood. This fact proves that the consumption of oxygen by muscles is increased during their contraction.

I conclude from the preceding facts :—

- 1st. Nervous action is not necessary for nutrition.
- 2d. Atrophy in palsied limbs is more a consequence of absence of exercise than of any other cause.
- 3d. Muscular atrophy, at any stage, may be cured by galvanism.

b.—Influence of the nervous centres on nutrition and secretion.

1. Every one knows the singular alterations which take place in the eye after a contusion of the frontal nerve, or a section of the trigeminal or the cervical sympathetic nerves. Every one knows also that the existence of worms in the intestinal canal, and also certain affections of the spinal cord, are able to produce morbid phenomena in vision, and even diseases of the eye, and especially amaurosis. I have found that after the section of a lateral half of the spinal cord, it sometimes happens that the eye, on the same side where the cord has been wounded, will present strange and various alterations. The part of the cord having that influence on the eye, lies between the ninth and the twelfth costal vertebræ. The alteration exists generally in the cornea. In one case a ridge appeared on the anterior surface of that membrane four days after the operation. On the fifth day the ridge was deeper, and its edges had become opaque; on the sixth day all the cornea was opaque. It remained so for

five days, after which the opacity disappeared and no trace remained of it, or of the ridge. This experiment has been made on guinea-pigs.

2. I have found a considerable hypertrophy of the two supra-renal capsules, on eight or ten guinea-pigs, upon which a lateral half of the spinal cord had been cut in the dorsal region, for eight, ten or fifteen months. These organs had acquired, in some of these cases, three times their natural dimensions, and in others only the double. There was no appearance of change in their structure.

By an examination of the supra-renal capsules in guinea-pigs, on which I had made the section of a lateral half of the spinal cord, a few hours or a few days previously, I have found these organs congested, and sometimes containing even a slight effusion of blood. It is very probable that such a congestion has been the cause of the hypertrophy found in animals operated on at a much longer time previously. The congestion is certainly the result of a peculiar disturbance in the nervous action. A part only of the spinal cord appears to possess that singular influence on the supra-renal capsules. That part is extended, in guinea-pigs, from the tenth costal vertebra to the third lumbar.

A simple puncture of the cord is frequently sufficient to produce the congestion of both supra-renal capsules.

3. The researches, made before mine, as to the influence of the spinal cord on the urinary secretion, could not give a decided result, because no physiologist had been able to keep any warm-blooded animal living a sufficient time, after the destruction of a large part of the spinal cord.

The results obtained by Ségalas on some animals who have lived from fifteen minutes to an hour after the destruction of the lumbar part of the cord, had led him to conclude that the spinal cord has no influence on the urinary secretion. Longet (*Traité de Physiologie*, Paris, 1850, t. ii. B. p. 199) says:—"Many observations have demonstrated to me that the visceral organs, which receive their nerves from the sympathetic, are far from being immediately paralyzed by the section of these nerves, and that their action is even maintained much longer than the duration of the experiments in which Ségalas had destroyed the spinal

marrow.* Therefore I think I am allowed to maintain that after such an injury, the nerves going to these organs, and more particularly to the kidneys, do nothing but spend little by little the *nervous force*, originally and principally derived from the spinal marrow, which is the chief, if not the exclusive centre of its production; thence the persistence of the renal secretion, as well as that of the movements of the heart, the intestinal canal, the uterine cornua, etc."

I could relate a great many experiments proving the incorrectness of Longet's theory, but a single one is sufficient. I have kept living, nearly *three months*, a young cat, on which the spinal cord had been *completely destroyed* from the eleventh or twelfth costal vertebra to its termination. This cat has lived all that time in apparently good health, and its *urine* has always been perfectly normal. It was acid, as is the case constantly in cats fed on meat, milk and bread. The bladder was palsied, but the sphincter vesicæ was generally contracted, so that every day I had to compress the abdomen and the bladder to empty this pouch. When I remained two days without doing that operation, the bladder contracted in consequence of the excitation produced on its muscular fibres by their distension.

This fact clearly proves that the urinary secretion is not under the dependence of the spinal cord.

According to Krimer, the medulla oblongata is the nervous centre upon which the urinary secretion depends. My experiments prove that this opinion is incorrect:—1st. After the destruction of the medulla oblongata in frogs, I have found that the secretion of urine remains as long as the animals have lived, *i. e.*, three or four months. 2d. On hibernating mammals, in winter time I have extirpated the medulla oblongata, after having emptied the bladder. These animals have lived a little more than a day, when I took the precaution of insufflating air in their lungs many times each hour. After their death the bladder was found full of urine apparently normal.

The medulla oblongata is not therefore a centre on which the urinary secretion depends.

4. The well known opinions of Segalas, W. Philip, Krimer,

* The italics are Longet's.

Chossat, Longet and others, about the influence of the spinal cord on the functions of organic life, are quite erroneous. I have found that birds are able to live for months after the destruction of the spinal cord, from the fifth costal vertebra to its termination. This fact proves not only that the functions of organic life may continue to exist in such a case, but that they appear to be executed then as in healthy birds; for, if the operation has been made on a young bird, it will afterwards grow very well.

I have succeeded in keeping alive, from the 8th of April until the 4th of July, a young cat, about which I have already published a note in this journal.* The palsied parts in this animal have grown in length proportionately as much as the sound parts. The growth has been such in the palsied limbs that they have acquired more than double the length they had at the time of the operation. The functions of organic life appeared to exist without any apparent disturbance. The nutritive reparation was so powerful, that the pieces of the vertebral column which had been cut off have been reproduced. This fact is important, because it shows that the reproduction of bone is possible in a palsied part.

The temperature of that cat was at the ordinary degree, (105° Fahr., in the rectum.) The secretion of the hair and nails took place as in healthy cats. I had previously seen on birds that their temperature remained normal after the destruction of a great part of the spinal cord. Besides, I have found in these birds that the secretion of quills and nails continued to take place.

As to the influence of the medulla oblongata on the functions of organic life, my experiments on cold-blooded vertebrata have proved to me, that these functions (except, of course, pulmonary respiration,) may continue to exist without any appearance of disturbance.

5. After the complete transverse section of the spinal cord in mammals or birds, I have found that the ulcerations which take place around the genital organs do not result directly from the absence of nervous action. One of the causes of these ulcerations is continued pressure, and another cause is the continual presence of altered urine and fæces.

* See Med. Exam., No. v. May, 1852, p. 321.

My opinion is well proved by the following experiments:—

1st. I have put, three or four times a day and for many days, a certain quantity of urine on the posterior part of the neck, in the neighborhood of the scapulæ, upon guinea-pigs. Before a week elapsed, the skin, at the place acted on by the urine, had lost its hair and epidermis. After a week more there was an ulceration in the skin, and ten or twelve days later the skin was destroyed, and there was an ulcer with a very bad aspect. This fact proves how powerful is the action of urine on the skin.

2d. On guinea-pigs, upon which the spinal cord was cut in the dorsal region, and on pigeons, upon which the spinal cord was destroyed from the fifth costal vertebra to its termination, I have found that no ulceration appeared when I took care to prevent any part of their bodies from being in a continued state of compression, and of washing them many times a day to remove the urine and fæces.

3d. In cases where an ulceration had been produced, I have succeeded in curing it by washing and preventing compression.

4th. I have found that in animals having the spinal cord cut across, every kind of wounds or burns were cured as quickly as in healthy animals.

Therefore the ulcerations which appear, in cases of paraplegia, do not exist directly in consequence of the palsy; they can be avoided and in many cases they can be cured.

These conclusions are perfectly true in animals having had an injury to the spinal cord for a shorter time than a year; but on guinea-pigs, upon which a lateral half of the spinal cord, had been cut for fourteen, fifteen, or eighteen months, near the tenth or eleventh costal vertebra, I have found an alteration of nutrition in the palsied parts. It was the right half of the spinal cord which had been cut, and in such a case, as I have discovered, the left side of the body behind the wounded part evidently loses a portion of its sensibility, and its temperature is also diminished. I have found, at the time designated, an ulceration coming in the part between the sacrum and the hip-joint. That ulceration has taken a tolerably great extension in surface but not in depth. It became as large as a half dollar. The part ulcerated has never been subjected to any kind of compression, neither to the action of urine and fæces.

Another kind of disturbance of nutrition occurred in these animals: they lost the hair of the leg, and of the other parts in which the sensibility was diminished.

6. It is known that erection is a frequent phenomenon in men after a fracture or a luxation of the vertebral column. It is known also, that in men hanged, erection and even ejaculation are not uncommon. Ségalas says he has seen these phenomena produced by the excitation of the spinal cord. Longet (*loc. cit.*, p. 201,) declares that he has not seen the excitation of the cord producing such effects. It is very easy to ascertain, on male guinea-pigs, that Ségalas is right.

1st. A transverse section of the spinal cord always produces an erection and an ejaculation.

2d. When one of these animals is asphyxiated, erection and ejaculation take place.

3d. If the spinal cord is galvanized, erection and ejaculation are produced.

These facts prove the influence of the spinal marrow on the seminal vesicles. They empty themselves slowly when the cord is galvanized.

In asphyxia, there are universal convulsions even in the muscles of organic life, as uterus, intestine, etc. These last organs are then put in contraction, and it is not astonishing, consequently, that the seminal vesicles become also contracted. The cause of these general contractions is the excitation of the spinal cord by venous blood, and very probably by a large amount of carbonic acid, as I will elsewhere try to demonstrate.

IV.—ON THE REPARATIVE POWER OF THE NERVOUS SYSTEM.

I have recently published in this journal* the results of my researches on the reparative power of the spinal cord. From these researches I have drawn the following conclusions:—

1st. That the spinal marrow, even in adult mammalia, may be exposed to the action of the air without danger to the life of the animal.

2d. That wounds of the spinal marrow may be repaired.

* *Med. Exam.*, No. vi., June, 1852, p. 379.

3d. That after a complete transverse section of the spinal cord, the functions of that organ may be entirely restored.

As to the nerves, the experiments of Fontana, Haighton, Tiedemann, Flourens, Steinrueck, and many others, have demonstrated the possibility of reunion of the two extremities of a cut nerve. But, in most, if not in all these experiments, the return of sensibility and of voluntary movements have not been complete. The following fact is, consequently, very important, because it proves the possibility of a complete reappearance of the lost faculties after the entire division of a nerve.*

A guinea-pig, on which the sciatic nerve had been cut across, exhibited indications of a return of sensibility a month after the operation. Two months afterwards the sensibility was increased, but was still much inferior to that of the sound limb. The muscles then were beginning to contract under the influence of the will. Six months after the section, the animal could evidently move its legs and toes voluntarily; the sensibility then was almost entirely recovered. At the end of about eleven months, the sensibility and all the voluntary movements were apparently alike in the two posterior limbs. The animal having been killed, it was found by my friend Dr. Lebert and myself that, except a slight union of muscular fibres with the nerve at the place where it had been divided, the restoration of the original condition was so complete that no indication of the division could be discovered, either with the naked eye or with the microscope. I had seen the usual swelling of the nerve at the point of reunion about the sixth month after the operation, but at the time of the last examination it had disappeared.

V.—ON TURNING AND ROLLING AS PHENOMENA PRODUCED BY INJURIES OF THE NERVOUS SYSTEM.

Pourfour du Petit and Mécée de la Touche were the first experimenters who witnessed turning produced by an injury of the nervous centres. But the first valuable researches on this phenomenon were made by Magendie and Flourens.

The parts of the cerebro-spinal centre which can be injured without producing turning, are: the cerebral hemispheres, the

* See Gaz. Med. de Paris, 1849, t. iv. p. 880.

cerebellum, the corpora striata, the corpus callosum, the spinal marrow and the olfactive and optic nerves.* All the other parts of the cerebro-spinal centres are able to produce turning or rolling.

These circulatory or rotatory movements take place sometimes on the same side, and sometimes on the side of the body opposite to that of the encephalon which has been injured.

A puncture of one of the following parts produces turning or rolling on the injured side :

1st. The anterior extremity of the thalami optici, according to Schiff.

2d. The crura cerebri, according to Magendie.

3d. The bi, or quadri-geminal tubercles, according to Flourens.

4th. The pons varolii.

5th. The posterior part of the processus cerebelli ad pontem.

6th. The auditive nerve, according to my own experiments.

7th. The medulla oblongata at the point of insertion of the facial nerve, according to my experiments in common with Dr. Martin-Magron.

8th. The medulla oblongata outside of the anterior pyramids, according to Magendie.

9th. A great part of the posterior face of the medulla oblongata, according to my experiments.

The parts of the encephalon which produce turning or rolling on the opposite side, are :

1st. The posterior extremity of the thalami optici, according to Schiff.

2d. The crura cerebri, according to Lafargue.

3d. The anterior part of the processus cerebelli ad pontem.

4th. A small part of the medulla oblongata before the nib of the calamus scriptorius and behind the corpora olivaria, according to my experiments in common with Dr. Martin-Magron.

Some of these two series of parts ordinarily produce turning and the others rolling. But these two kinds of movements can be produced by the puncture of a single part of the encephalon. Rolling is nothing but the exaggeration of turning ; thus, after

* I consider as a part of the nervous centres, the three nerves of the superior senses : the olfactive, the optic and the auditive.

a puncture of the medulla oblongata, the animal at first rolls, and after some instants, instead of rolling, it turns. If, when it is turning, a slight puncture is made anew, close to the first, then the animal rolls.

Before trying to explain turning, I will give an outline of some of its species.

1st. *Turning and Rolling caused by tearing the facial nerve.*

My friend Dr. Martin-Magron and myself have discovered that if the facial nerve of a rabbit or a guinea-pig be exposed at its exit from the stylo-mastoid foramen, and be then drawn away from the cranium, so as to tear it asunder near its origin, the animal begins in about five minutes to turn itself round and round, the movement being from left to right when the nerve has been thus torn on the left side, and from right to left when it has been torn on the right side. This rotation is generally preceded by convulsive movements of the eyes, of the jaws, and of the head upon the trunk: and the body is then bent (as in pleurosthotonos) towards the injured side, by the contraction of all the longitudinal muscles of that side, the power of which is such as to resist considerable force applied to extend them. The movement at first takes place in a small circle; but the circle generally enlarges more and more, until at last, after twenty or thirty minutes, the animal walks in a straight line. There is no paralysis of any muscles, save the facial. The effect is not produced, unless the nerve be torn close to its origin.

When the nerve on the other side also is torn, even after a long interval, instead of the tendency to turn to one side, there is, at first, a rolling of the body on its longitudinal axis, which takes place towards the side last operated on. After this has continued, however, for twenty minutes or more, the animal recovers its feet, and begins to *turn*, as after the first operation, but towards the other side. This movement soon ceases.

Dr. Martin-Magron and myself think that the cause of these phenomena does not exist in the facial nerve itself, but in the part of the medulla oblongata from which this nerve has originated.*

* See Gaz. Méd. de Paris, 1849, t. 4, p. 879.

2d. *Turning and Rolling produced by an injury to the Medulla Oblongata.*

M. Magendie (*Précis Elém. de Physiol.* Paris, 1836, t. 1, p. 414) says: "Having raised up the cerebellum, I make a section perpendicularly to the surface of the fourth ventricle and at three or four millimetres from the median line. If I cut on the right, the animal will turn on the right side; if I cut on the left, it will turn on the left side."

If we suppose a plane cutting the medulla oblongata transversely at the distance of nearly two lines before the nib of the calamus scriptorius, the posterior face of the medulla oblongata will be divided into two parts: one before that plane, which I will call superior, and the other behind, or inferior.

Now, every puncture on that superior part produces turning or rolling on the side which has been punctured. The slightest puncture on the processus cerebelli ad medullam oblongatam is able to produce a violent and very rapid rolling. As long as the animal lives after the operation, it rolls or it turns at each time it tries to walk.

When (as Dr. Martin-Magron and myself have discovered) a deep section is made on the inferior part of the posterior face of the medulla oblongata, before the nib of the calamus scriptorius, turning is produced on the side of the body opposite to the punctured side of the medulla. A rabbit, which has lived thirteen days after the operation, had still the circulatory movement a few hours before dying. Nevertheless, sometimes the animal could walk nearly straight during a few seconds.

3d. *Turning Produced by a Puncture or a Section of the Acoustic Nerve.*

Flourens has discovered that, after the section of the semi-circular canals, turning sometimes takes place.

I have found all the facts he relates about this subject perfectly right. It was interesting to know if a puncture or the section of the auditive nerve would produce turning. As it was impossible to operate on that nerve in mammals, I have experimented on frogs: In these amphibia it is easy to find the nerve and to act upon it. I have found that after a puncture or a section on the trunk of the nerve, the animal begins instantly to turn. As

long as the frogs live, after a puncture of the acoustic nerve, they turn; but the circle of turning is much smaller a short time after the operation than afterwards. I have kept such frogs for months.

4th. *On a New Mode of Turning.*

I have found a mode of turning which has altogether some of the characters of turning and of rolling.

In the circulatory movement called turning (*mouvement de manège*), the body of the animal is bent on one of the lateral sides. It has the shape of an arch, and this arch is generally a part of the circumference described by the animal when turning. The smaller the radius of that arch, the smaller is the circle of turning.

In the new mode of turning I have found, the body of the animal is not bent, and when it walks it moves laterally, instead of going forwards. In turning it describes a circle, but the longitudinal axis of its body, instead of being then a part of the circumference, is a part of a radius, so that its head is at the circumference, and its tail towards the centre of the described circle.

That mode of turning has been executed by animals on which the quadrigeminal tubercles and the pons varolii, on one side, had been punctured by a pin. One of the eyes was convulsed; the other was in its normal condition. The convulsed eye was the right one, and the tubercles punctured were those of the left side.

5th. *On the Causes of Turning and Rolling.*

I have not room enough to show that the theories of Magendie, Flourens, Henle, Lafargue and Schiff are contradicted by a great many facts. I will only present the following remarks:

1st. As the slightest puncture of certain parts of the encephalon is sufficient to produce turning or rolling, it is evident that those rotating movements do not exist in consequence of an hemiplegia, as Lafargue, Longet and Schiff believe they do. Another reason is that every degree of hemiplegia exist in man without being accompanied by turning or rolling. Besides, these phenomena have been observed in persons who had no paralysis at all.

2d. The theories of Magendie and Flourens are also opposed, by the fact that a slight puncture is sufficient to produce turning or rolling.

3d. As to the theory of Henle, which is based upon the existence of convulsions in the eye, producing a kind of vertigo, it has against it the facts that, on one side, convulsions may exist in the eyes without any other disorder in the movements; and, on the other side, sometimes turning or rolling exist without any convulsion in the eyes.*

Nevertheless, I think that, in many cases, the vertigo consequent on convulsions of the eyes is one element of the cause of turning. I think also that, in certain cases, paralysis of some parts of the body may facilitate the rotatory movements. But their great cause, I think, is the existence of a convulsive contraction in some of the muscles, on one side of the body. These convulsive contractions are to be found in every case of circulatory or rotatory movement. As to the cause of these contractions, it exists in the irritation produced in certain parts of the encephalon.

VI.—ON A MEANS OF MEASURING DEGREES OF ANÆSTHESIA AND HYPERÆSTHESIA.

The curious facts discovered by E. H. Weber, on tactile sensibility, are well known. He found that if the two blunted points of a pair of compasses are applied simultaneously on the skin, there is, according to certain circumstances, either the sensation of one or of two points. When the points are both inside of certain boundaries, they are felt as one only; when they are outside of these boundaries, both are felt. These boundaries vary exceedingly in different parts of the skin, but for a given part the differences between men are not extremely considerable. I have made use of the compasses for measuring the degrees of tactile sensibility in diseases. In a case of considerable anæsthesia of the lower extremities, the patient only felt a single impression on one leg, although the

* See a very remarkable case observed by my friend Dr. Lebreton, in *Comptes rendus et Memoires de la Soc. de Biologie* année 1850. Paris, 1851. t. ii. p. 7.

points of the compasses were ten, fifteen, or even twenty centimetres apart; whilst on the other leg he could distinguish them at a distance of twelve centimetres. The normal limit is generally, for that limb, from three to five centimetres. In another case where anæsthesia was slighter, the limit of the discriminating power was at from nine to sixteen centimetres. In two other cases, in which the diminution of sensibility had not been found by the other means of diagnosis, the compass indicated a very slight and beginning anæsthesia. The limit was at from six to seven centimetres.

These facts, and many others, have demonstrated to me that by the help of the compass, a physician can ascertain : 1st. Whether there is a slight anæsthesia or no. 2d. What is the degree of anæsthesia. 3d. What changes occur every day in the amount of anæsthesia.

The same is true for the cases of hyperæsthesia. In a case of paraplegia of the motor power, the patient felt the two points of the compasses, on his feet, even at the distance of five millimetres, whilst a healthy person feels the two points only when they are at a greater distance than twenty-five millimetres.

I shall add, that for succeeding in such experiments the two points must be blunted and applied simultaneously.*

* See Gaz. Med. de Paris, 1849, t. iv. p. 1012.

VII.—ON THE CAUSES OF THE TORPIDITY OF THE TENREC, (*Erinaceus ecaudatus*, Linn.)

The influence of cold as a cause of hybernation has not been considered essential, on account of an assumed fact, which is altogether incorrect. This fact is that a mammal called the Tenrec, (*Erinaceus ecaudatus*, Lin.), which lives in the Islands of Mauritius, of Madagasear and Bourbon, is torpid under the joined influences of warmth and dryness. Cuvier says, about the Tenrec: "They are nocturnal animals, remaining in lethargy three months each year, although they inhabit the torrid zone. Moreover, Bruguiere asserts that it is during the time of the greatest heat that they are torpid." Physiologists have admitted this fact as true, and they have supposed that warmth and dryness could be, like cold, a cause of torpidity.

The following facts prove that the torpid sleep of the tenrec takes place from precisely the same cause as that of the hedge-hog, (*Erinaceus Europeus*, Lin.,) and of other hybernating mammals:

1st. The tenrec and the hedge-hog belong to the same family, and they are much alike.

2d. According to MM. Desjardins, Telfair and Coquerel, and to my own observations, the tenrees earth themselves, and are torpid from the month of June to the month of November, *i. e.* during the winter season of the Islands where they live.

3d. Hybernating animals, belonging to varied species, observed by Pallas, Mangili, Marshall Hall, Berthold, Barkow, and myself, have been found torpid at the temperature of 61 to 66° F., (16 to 19° Cs.) Moreover, I have found that dormice, (*Mus glis*, Lin.) even at a temperature of 63 to 72° F., (20 to 22° Cs.) may become torpid, and I have observed some that were constantly sleeping, during a whole week, at a temperature varying from 59 to 68° F. (15 to 20° Cs.) I have stated that hedge-hogs may be torpid during the summer; in Paris, at a temperature of 68 to 72° F., (20 to 22° Cs.) and lately, in Philadelphia, I have seen a marmot (*Arctomys monax*, Buff.) torpid, in June, at a temperature of 71 to 73° F. (21.5 to 23° Cs.)

4th. During the time of its torpidity, the tenrec is under

the influence of a temperature of 59 to 72 or 73° F., (15 to 22 or 23° Cs.) rarely more, and sometimes less. Therefore these animals are exposed to a temperature sufficiently low to render them torpid, for that temperature may produce that effect on the hibernating animals of cold countries.

From these facts I believe it is right to conclude that torpidity is induced in the tenrec, in the same way as in the other hibernating mammals, and therefore it is not necessary to suppose that warmth and dryness produce torpidity.

VIII.—ON THE INFLUENCE OF POISONS UPON ANIMAL HEAT AS A CAUSE OF DEATH.

Prévost and Chossat, and after them, M. Magendie, have ascertained that death occurs quickly in mammals when their temperature is notably diminished. My experiments confirm the correctness of that statement. The diminution of animal heat in mammals, is so dangerous that, in one case, I have seen death take place in a rabbit after a diminution of only 22° F. (12° Cs.) I have never observed any animal continuing to live when I had diminished its temperature more than 44° F. (24°·5 Cs.) I have found the law established by Chossat perfectly correct, according to which the diminution of animal heat necessary for killing is less and less, in proportion to the rapidity with which that diminution takes place.

It is very probable that in all the cases where, in consequence either of disease, or of a wound, or of poison, the temperature of man is diminished many degrees, his life is in danger from the very fact of that diminution. It is thus in cholera, in sclerema, in certain cases of palsy, in cases of great disturbance of the respiration, in fractures and luxations of the vertebral column, in the cervical, and even in the dorsal regions, in cases of profuse hæmorrhage, and in many cases of poisoning when death is not rapidly produced.

It has been long known that temperature is diminished in poisoned persons; and there are but few cases of poisoning on record in which it is not said that the patient was cold. Chossat has found that a dog, into whose veins he had injected opium, had its temperature diminished from 105° to 62·6° F. (40°·3 to 17° Cs.), 22 hours after the injection. Brodie has

found that many poisons act upon animal heat so as to diminish it considerably. Demarquay and Duméril, junior, and later, these two experimenters, joined with Lecointre, have found the same thing as Brodie in many toxic agents. I have made very numerous experiments on this subject, and some of their results have been published before the last papers of Demarquay, Duméril and Lecointre.*

I have stated that many poisons, either injected in the veins or absorbed by the vessels of the skin or of the digestive canal, may diminish sufficiently the temperature of Guinea pigs and rabbits, to produce death. This occurs when the dose of the poison is not large enough to kill in less than four or five hours. These poisons may kill only by their action upon animal heat. It may be so with opium, cyanhydric acid, the cyanide of mercury, hyoscyamus, digitalis, belladonna, tobacco, euphorbia, camphor, alcohol, acetic, oxalic, sulphuric, azotic, chlorohydric acids much diluted, and some oxalates.

Of course the action of these poisons is the greater, the colder the atmosphere; but it is not always immediately so, and instead of diminishing the animal heat, many may increase it for a time, especially when the temperature of the atmosphere is elevated.

I have discovered that a dose of one of these poisons, sufficient to kill an animal, when there is no obstacle to the diminution of its temperature, may be unable to destroy life when the temperature of the animal is maintained by artificial means to its normal degree, or not far from it. My experiments have been conducted as follows:

Equal doses of poison were given, simultaneously, to two animals, as much alike one another as possible. One of them was left in a room at a temperature of from 46 to 50° F. (8 to 10° Cs.), and the other was kept not far from a chimney, in a place where the air was at from 75 to 86° F. (24 to 30° Cs.) The first was dead after a certain number of hours, or sometimes one or two days, having its temperature much diminished. The other, on the contrary, had no perceptible diminution of its temperature, and was generally cured very soon. Therefore,

* See Gaz. Med. de Paris, 1849, t. iv., p. 644.

when taken in certain doses, many poisons may kill only by their influence on animal heat, and physicians, in cases of poisoning, should try as much to prevent the diminution of temperature, as to expel the poison or to act against it by an antidote, by pulmonary insufflation, or otherwise.

In experiments which I have made lately on the action of very pure digitaline, that had been prepared by M. Quévenne, the celebrated chemist, who has made such interesting and accurate researches on digitalis and the substances of which it is composed, I have found that this poison may also diminish temperature. I believe it is easy to explain the contradiction existing between Traube and Stannius, as regards the influence of digitalis on animal heat. When the atmosphere, in which the animal is, is cold, then its temperature may be diminished by digitalis or digitaline, but when it is warm the diminution does not take place, or it is very small. But, of course, if the dose is sufficient to kill very quickly, then it is indifferent whether the atmosphere is cold or not, because there may be not time enough for the diminution of the temperature of the animal.

I have to relate another fact which, I believe, ought to be considered as analogous to the preceding. It is that kind of poisoning which occurs when a layer of oil, of varnish or of gelatin, is put on the skin of a warm-blooded animal. Death, then, is very probably produced by a substance unknown until now, and which is secreted by the skin. The layer of oil, varnish, or gelatin preventing that secretion taking place, that unknown substance becomes accumulated in the blood, and then are produced the phenomena so well studied by MM. Fourcault, Becquerel, Breschet, and Magendie. I have found that in such a case the animals may live, if the atmosphere in which they are kept is at a temperature inferior to 79 or 80° F. (26 or 27° Cs.) In these circumstances their temperature is not sensibly diminished, while it diminishes much when the atmosphere is cold. Therefore it is especially by their loss of warmth that animals are killed, when their body has been entirely covered with oil, varnish, or gelatin.

IX.—ON CERTAIN ACTIONS OF COLD, WARMTH, AND LIGHT UPON THE
CRYSTALLINE LENS.

Pourfour du Petit has discovered that after the death of a mammal, it is not uncommon to find the crystalline lens opaque. He has found also, that when the lens has become opaque, if we draw it near the flame of a candle or a lamp, it becomes transparent again after a few minutes. The same lens, put alternately near and far from the flame, may become alternately transparent and opaque. I have endeavored to discover whether it is the light or the warmth of the flame which renders an opaque lens transparent. I have placed between the lens and the flame a layer of mineral salt, which is athermane and transparent. Then the light of the flame could reach the lens, but not its warmth. There has been no action. In another experiment I have exposed to the light passing through the mineral salt a fresh crystalline lens, still perfectly transparent. It became opaque. Now, in a third experiment, I have compared two crystalline lenses, perfectly transparent, one exposed to the action of light passing through the mineral salt, and the other kept in an obscure place. The former became opaque much quicker, and considerably more than the other. Therefore, 1st. It is not light which renders transparent an opaque lens; 2d. Light does not prevent a transparent lens from becoming opaque; 3d. Moreover, light appears to accelerate, if not to produce, the opacification of the crystalline lens.

As to the influence of warmth, it is certain that it is that which renders transparent an opaque lens: 1st. When left in the atmosphere, at a temperature superior to 70° F. (21° Cs.), a fresh transparent lens remains transparent. 2d. When a lens has become opaque, it is frequently sufficient to keep it exposed to the warmth of the hand, during some minutes, to render it transparent. 3d. The lower the temperature of the atmosphere the quicker transparent lenses become opaque.

These experiments give a very interesting result, *i. e.*, that light appears able to produce an effect precisely opposite to the effect produced by warmth.

From these researches I conclude :

1st. That light, and a low temperature, are favorable condi-

tions, if not direct causes, of the opacity of the lens in warm-blooded animals after death.

2d. That a warm temperature, *i. e.*, a temperature superior to 70° F. (21° Cs.) is a cause of the change occurring in the lens, by which, when opaque, it becomes transparent.

I will add, that sometimes an analogous opacity takes place in the cornea.

X.—ON THE NORMAL DEGREE OF THE TEMPERATURE OF MAN.

The degree of animal heat, in the human species, is stated as being between 98.5° and 100° F. (37 and 38° Cs.) I intend to prove that it is higher.

It is impossible to take directly the temperature of most of the internal parts of the body in man; therefore many physiologists, in order to discover the temperature of these parts, have argued as follows: According to J. Hunter, the temperature of the rectum in animals is the same as that of the right ventricle of the heart; hence it has been concluded that as the temperature of the rectum, in man—still according to Hunter—is 98°.42 F. (36°.9 Cs.) the temperature of the internal parts of the body ought to be between 98 and 99° F. (36°.67 and 37°.22 Cs.)

Some other physiologists, noting the temperature of the mouth under the tongue, and supposing that this temperature is nearly the same as that of the internal parts of the body, have concluded that the temperature of man is between 99 and 100° F. (37°.2 and 37°.8 Cs.)

We will prove that these deductions are not right:

Firstly, the degree of the temperature of the rectum is not, as Hunter says, 98°.4. It may be so in debilitated men, but in healthy persons it is more elevated. It is between 100 and 102° F. (37.7 and 38°.89 Cs.) according to my own researches and to those of Berger and Maunoir. Besides, many experiments on dogs, rabbits and guinea-pigs, have shown satisfactorily to myself that the temperature of the rectum is not equal to that of the right ventricle. This last organ is from 1 to 3° F. (0.56 to 1.7° Cs.) higher than the rectum.

The temperature of the rectum, in man, being between 100 and 102° F. (37°.7 and 38°.8 Cs.), and the temperature of the

right ventricle, in man, being from 1 to 3° F. (0.56 to 1.7 Cs.) higher than that of the rectum, if we suppose the same difference existing in man as in mammals, it follows that the temperature of the right ventricle of the heart in man ought to be between 101 and 105° F. (38°.33 and 40°.56 Cs.)

But a closer approximation of the exact temperature of the internal parts of the body, in man, may be obtained by taking the temperature of an organ situated deeper than the rectum, and, consequently, less exposed to the influence of the atmosphere. Such is the case in the bladder. I have observed its temperature by taking that of the urine at the moment of its emission and before any sensible change had occurred in it. The urine was directly received in a vase dipped into a large quantity of water at 98° F. (36°.7 Cs.) Thus I have ascertained that the mean temperature of the urine, in man, is 102°.6 F. (39°.2 Cs.)

Now if we take notice of this well-known fact, that the temperature of the lower part of the abdomen is less elevated than the upper part, we are authorized to believe that the temperature of the central parts of the body, in man, is very near 103° F. (39°.5 Cs.)

My experiments on the temperature of urine were made on ten strong sailors, in the spring, on the Atlantic Ocean, between the 43d and 45th deg. of north latitude. The lowest degree of the temperature of urine which I have observed, was 100°.9 F. (38°.3 Cs.); the highest was 103°.2 F. (39°.56 Cs.) My own urine, examined more than thirty times, in the most varied conditions, has been nearly always at the same temperature; the variations have been only between 102° and 102°.8 F. (38°.89 and 39°.33 Cs.) The ordinary degree is 102°.5 F. (39°.17 Cs.)

Long before my researches, the temperature of urine, in the human species, had been taken by some observers, but in general without sufficient care. The temperature of the urine is 94°.25, according to Braun; 98°.9, according to De Lisle; and 103°, according to Hales. Recently Berger has taken the temperature of urine in the bladder in five women. He has found it equal to 101°.48 F. (38°.6 Cs.)

As the temperature of woman is a little inferior to that of man, the result obtained by Berger is in accordance with mine.

If we take the average between the temperature of the urine of

man as I have found it, and that of woman as Berger has found it, we have a number very near 102° F. ($38^{\circ}.9$ Cs.)

From these facts we draw the conclusion that the temperature of the thoracic and abdominal viscera, in the human species and in both sexes, is between 102 and 103° F. ($38^{\circ}.89$ and $39^{\circ}.44$ Cs.), *i. e.*, some degrees higher than it is generally admitted.

XI.—ON THE INFLUENCE EXERTED UPON THE GENERAL TEMPERATURE OF THE BODY BY A CHANGE IN THE TEMPERATURE OF ONE OF THE EXTREMITIES.

The following sentence is given, as an axiom, by Dr. W. F. Edwards: "We cannot either raise or lower the temperature of any one part of the body, without all the other parts of the frame being affected and suffering a corresponding rise or fall in temperature, more or less, according to circumstances."*

Expressed in such terms, we accept this law as perfectly true. But the author elsewhere gives an extension to this law, which we will prove to be incorrect. No doubt when the temperature of the blood, coming to the heart from a remote part of the body, has been modified in that part, the thoracic viscera ought to have their temperature modified; but to what extent? It is on this very important point that we do not agree with Dr. Edwards. He was of opinion that the influence exerted by a small part, on all the other parts of the body, was considerable. He says that the chilling of a single part, such as the hand or the foot, may cause a loss of temperature in all the other parts of the frame, even far beyond what could have been presumed as likely or possible, and that in a number of experiments where one hand was plunged in water cooled down by ice, the other hand, which was not subjected to the action of the cold bath, lost nearly 5° R. ($11^{\circ}.25$ F., $6^{\circ}.25$ Cs.) in temperature.

It will be easily understood how important it would be that practitioners should be able to act on the general temperature of a patient, so as to increase or diminish it, by means of a hand or a foot bath. I regret to say that if the facts observed by Edwards are exact, his conclusion nevertheless is incorrect.

* Article *Animal Heat*, in Todd's *Cyclop. of Anat. and Physiol.*, 1839, t. ii., p. 660.

I have performed, both alone, and with the help of Dr. Tholozan, Physician of the Hospital *Val-de-Grace* at Paris, numerous experiments, with the view of knowing whether Dr. Edwards was right or not. I have found that the chilling of one hand plunged in water at the temperature of freezing point, acted very strongly on the temperature of the other hand. But, at first, there is no regularity at all in the quantity of degrees of temperature lost by the hand which remains out of the water; and secondly, we have found once that this hand did not lose any fraction of its temperature. In one case we have observed that the hand kept in the atmosphere did lose 22° F. (12° Cs.) in seven minutes. The ordinary loss of temperature has been of between 6 to 8° F. (3.33 to $4^{\circ}.44$ Cs.) In one case there has been only a loss of 2° F. ($1^{\circ}.2$ Cs.) In another case there has been no loss, and, on the contrary, there has been an increase of temperature of $1^{\circ}.4$ F. ($0^{\circ}.8$ Cs.)

If, like Edwards, we consider the loss of temperature of the hand not plunged in water as a sign and as a measure of the diminution of the general temperature of the body, we must conclude, from our experiments, that the chilling of a small part of the body may be unable to diminish the temperature of the body sensibly, and that, in other cases, it may act extraordinarily upon it. But the supposition that the hand kept in the atmosphere, was able to give a measure of the modification of the body is incorrect. By taking the temperature of the mouth during the time that one hand was dipped into very cold water, Dr. Tholozan and myself have ascertained that the temperature of the body does not sensibly change. The greatest diminution of the temperature of the mouth has been nearly 1° F. ($0^{\circ}.6$ Cs.), and this only in one case. In that experiment in which the hand not plunged in water lost 22° F. (12° Cs.), the temperature of the mouth was not diminished more than the fifth of a Fahr. degree.

As it is quite certain that the temperature of the body is constantly changing, it is easy to understand why we do not find a small but a constant diminution in the temperature of the mouth when one hand is subjected to a notable chilling. When, under the unknown influences that are constantly modifying the temperature of the body, there is a tendency to increase that

temperature, then the tendency to its diminution originating from the chilling of one hand may exist without producing any effect. We have there two causes acting in opposite directions, and if they are equal they annihilate each other. If they are unequal, we perceive only the difference between them. When these two causes act in the same direction, then their efforts are added to one another, and it is probably in a circumstance of that kind that I have once found a diminution of 1° F. ($0.^{\circ}56$ C.) taking place in the mouth.

We have now to examine how the diminution of the temperature of a hand is produced when the other hand is dipped into cold water. *A priori* it is evident that the chilling of the hand kept in the air exists in consequence, either of the arrival of a cooler blood, or in the diminution in the quantity of blood. That hand being exposed to a cold air (for it is only in such a case that the experiments succeed) loses its temperature by the action of that cold air. At first the blood that arrives in the hand is not cooler, as Edwards had supposed it was. This we prove by the fact that the temperature is but very little changed in the mouth. The supposition then remains that the quantity of blood arriving in the hand is smaller than usual. This may happen by two modes, one of which is that the heart sends less blood, and the other that the blood-vessels of the hand are contracted and prevent, in part, the passage of blood. It is certain that the heart continues perceptibly to send the same quantity of blood. Therefore we are induced to admit that the hand's blood-vessels are contracted. But now what is the cause of that contraction? We will try to show that it is in an action of the nervous system. Every one knows that under the influence of a sensation or of emotion, the hands, and sometimes the feet, become cold. The nervous system, in consequence of that sensation or emotion, acts upon the blood-vessels and excites them to contract. The calibre of the visible vessels is sensibly diminished. The same phenomena take place in the two hands when one is dipped into very cold water. An exceedingly violent pain is felt, the nervous centres are strongly excited, and they act then as under the influence of an emotion. Dr. Tholozan and myself have observed that the greater the pain felt, the more the temperature was diminished in the hand left in the air.

As to the influence of partial heating Dr. Edwards relates the following experiment :

“The hand being immersed in water heated to the temperature of 34° R. ($108^{\circ}.5$ F.— $42^{\circ}.5$ Cs.) rose one degree of the same scale, and the temperature of other remote parts not immediately exposed to the influence of heat were found to have risen to a corresponding degree.”

I have repeated this experiment of Dr. Edwards, and I have found no evident elevation in the temperature of remote parts, as the mouth and the hand, not immersed in water.

I conclude, then from the facts contained in this note, that, in general, the temperature of the body is not sensibly modified by the chilling or the heating of a small part of the frame.

XII.—ACTION OF COLD ON THE COAGULABILITY OF BLOOD, AND PERSISTENCE OF LIFE IN FROGS AFTER LOSING HALF OF THE VENTRICLE OF THE HEART.

Dr. Marchal (de Calvi) and other physiologists, have recently asserted that cold diminishes the quantity of fibrine and consequently the coagulability of blood. I have observed a curious fact, which shows that blood may possess a very great coagulability although exposed to the action of cold. I have found that after the removal of the half of a ventricle, in frogs, the blood may coagulate at the surface of the wound and the animal may continue to live. After the mutilation has been made, the lips of the wound are drawn upwards and inside in consequence of the muscular contraction. The blood flows very abundantly, but its coagulation quickly begins, and a layer of solidified blood is soon formed on the entire surface of the section, and by this process the wound is rapidly obliterated. The hemorrhage has been frequently stopped in a few minutes.

This experiment is successful only in cold seasons, probably because the batrachia are able to bear much better a loss of blood at a low than at a high temperature. The pulsations of such mutilated hearts continue with regularity and strength, but the impulse given by the remaining of the ventricle ought to be diminished. Nevertheless the circulation of blood is accomplished well enough to allow the animal to live many months.

I have shown to the *Société de Biologie* at Paris, two frogs on which the wound of the heart was cicatrised after fifteen days.*

XIII.—ON A SINGULAR CASE OF ANIMAL GRAFT.

Every one knows the experiments by which the cock's spurs or many other animal textures have been grafted on the body of an animal, and especially on a cock's comb. I have succeeded in grafting the tail of a young cat on a cock's comb. I performed this experiment in France in 1850.

After having divided the tail of a young cat, I made a longitudinal section on a cock's comb, and I united these two parts one to the other, by stitching the cut surface of the cat's tail to the cut surface of the cock's comb. The skin of the cat's tail had been turned a little over itself, so that its internal surface was in contiguity with the cut surface of the cock's comb. Eight days after, I punctured the skin of the tail at a distance from the cock's comb, and blood escaped, so that it was evident that circulation was already established. The tail had been cold during all the day of the operation, but it became warmer gradually from the second day. The union appeared much advanced on the third or fourth day. The tail was entirely fixed on the eighth day.

Unfortunately, on the eleventh day, the cock had a fight with another cock, and the cat's tail was torn out from the ground on which it had been fixed. I was thus deprived of the opportunity of knowing what transformations should have taken place in the tail.

By examining it I found that all its tissues were fresh, and that its blood-vessels contained blood.

XIV.—ON A CONVULSIVE AFFECTION PRODUCED BY CONSIDERABLE INJURIES OF THE SPINAL CORD.

I have discovered that a very violent convulsive affection is the constant result of a considerable wound of the spinal cord, in certain animals. It is more especially in guinea-pigs in whom a transversal section of a lateral half, or a complete section of

* See Gaz. Méd. de Paris, 1850, t. v. p. 169.

the spinal cord has been made, that this is the case.* The section was made at the level of one of the last six dorsal vertebræ or the two first lumbar.

When the convulsive fit begins, the muscles of the face and neck are the first which contract. The convulsions occur alternately in all the muscles of the eye, the face, the tongue, the jaws and the neck; so that the cause of these movements ought to act on the parts of the encephalon, from whence the facial, the trigeminal, the hypoglossus, and the motor nerves of the eyes originate. The head of the animal is alternately bent on both sides of the body, and lastly the limbs are agitated in every direction. In the case of a section of the lateral half of the spinal cord, in the lumbar region or at the end of the dorsal region, three limbs only are strongly convulsed; the two anterior and one of the posterior—the one that is on the side of the body opposite to the side of the section of the spinal marrow. The other posterior limb has only very slight movements. When the spinal cord has been entirely divided in the dorsal or lumbar regions, the two anterior limbs only are convulsed, with the muscles of the face and neck. These convulsions may come without any exterior excitation; but it is very easy generally to provoke them by frightening the animal, or by pinching, burning or otherwise exciting it. The part of the body upon which a mechanical excitation acts more powerfully is the skin of the face or the neck. This convulsive affection has a great analogy with epilepsy; but it has also some distinctive features,—for instance, the animals, during the convulsions, do not appear to lose their consciousness, and they frequently cry when they are pinched.

The affection begins generally eight, ten or twelve days after the spinal cord has been wounded. The convulsions then are not very strong, and it is usually four or five weeks after the operation that the fits are violent and easily produced. Three or four months after, the violence and frequency of these convulsions are diminished; yet in two or three instances they have increased in strength and frequency more than a year after the operation. The affection may last a long time: in one case it still existed two years after the operation.

* See *Gaz. Med. de Paris*, 1850, t. v. pp. 651, 895.

Generally the fits last from five to fifteen minutes. The stronger they are the shorter is their duration. For an hour or two, and sometimes for one or two days after a violent fit, it is impossible to produce another one by any kind of excitation. When a fit takes place a short time after a violent one, it is always weak or very short.

I have made the following experiment on some guinea-pigs having this convulsive affection: I put them in a small box, so that they had but little room to move, and I gave them food in great abundance. They then ate very much and were deprived of exercise, and, in consequence of that mode of living, they had exceedingly frequent convulsions. One of them had a fit nearly every quarter of an hour. These fits sometimes were strong, but they did not last long. The others had fits three or four times every day. I changed their mode of living, and put them all in a large room where they had only a small quantity of food. The influence of this new regimen was nearly immediate; the same day the number of fits was diminished, and in the course of the third week afterwards they had only one or two fits; and before long some of them were cured.

I must relate also a very interesting fact analogous to the preceding. In several guinea-pigs and frogs, which had the spinal cord transversely divided, I have observed that when the animals remained very quiet during many days, instead of the regular reflex movements, they had a violent tetanic convulsion in their posterior limbs, when the skin of these limbs was pinched.

Such a tetanic movement never occurred in these animals when I excited the regular reflex movements every day.

I have no room in this *résumé* to try to explain the facts related; I will do it in a special paper. I will only add that all the readers of this note, who know the views of Dr. Marshall Hall about convulsive diseases, will remark how strongly the facts I have pointed out are confirmatory of these views.

XV.—ON THE RELATIONS EXISTING BETWEEN THE ORGANISATION OF NERVE TUBES AND THEIR VITAL PROPERTIES.

It is well known that when the nerve tubes are examined in a very fresh state, their contents, the *medulla*, or *white substance* of

Schwann, appear pellucid and homogeneous, and of a fluid consistence; but a kind of coagulation soon takes place in that medulla, making it easily distinguishable from the tube itself, solid, grumous and much less transparent. Water has the power of producing very quickly that kind of coagulation of the white substance of Schwann, and the more quickly the warmer it is.

It was interesting to ascertain whether a nerve-fibre keeps or loses its vital properties when its contents have been transformed by coagulation. I have performed two series of experiments in order to solve this question; one on the motor nerves, the other on the nerves of sensibility.

1st. After having amputated the two limbs of a frog, I laid bare a long portion of the sciatic nerve in both. Half an hour afterwards these two nerves were still able to act perfectly well. Then a microscopical examination of some fibres from one of these nerves demonstrated that the coagulation of the medulla had begun, but was not yet complete. Ten minutes later the nerves were still capable of acting, and the microscope showed that the coagulation was complete. A similar experiment repeated a great many times has always furnished the same result.

2d. After having divided the two sciatic nerves, not far from the knee-joint, in a living frog, I have dipped the central part of these nerves into water. After a few minutes, some fibres taken from one of them were beginning to coagulate; yet these nerves possessed their sensibility. Ten minutes after, there was a complete coagulation of the medulla, while the sensibility was very little if at all diminished.

I draw from these facts the conclusion that nerve-fibres are capable of acting nearly as well when their contents are coagulated, as when they are still liquid.

This result appears to be important, inasmuch as it contributes to show that the white substance of Schwann has not a great part in nervous action. Three reasons concur in demonstrating that the essential part, the truly active one—that which possesses the vital properties—is not the white substance of Schwann:

1. We see that the vital properties of the nerve-fibres do not

appear altered, although there is a very material change in this substance when it coagulates.

2. If such a change as that which takes place in this substance when it coagulates, existed in a part possessing the vital properties belonging to nerves, there should certainly be a movement or a sensation accompanying it, and yet there is none. This material change is certainly more considerable than the change effected by a slight mechanical or galvanic excitation, which is capable of producing a movement or a sensation.

3. The microscope has already proved that the white substance does not exist in all the nerve-fibres, and that it is wanting, for instance, in the very fine fibres.

Therefore if it be not the white substance of Schwann which is active in the nerves, and as it is not the *cylinder axis* which possesses the vital properties of the nerves, because it is wanting in many fibres, it results that it is the membranous layer, the paries of the nerve-tube, to which these vital properties belong, unless there is another substance, still unknown, and existing in the tubular canal of the nerve-fibre.

XVI.—ON THE PERSISTENCE OF LIFE IN ANIMALS DEPRIVED OF THEIR MEDULLA OBLONGATA.*

I. There is no part in the nervous system considered as more essential to life than the medulla oblongata. In the last few years many German physiologists have asserted that this nervous centre is the source of the rhythmical movements of the heart. Besides, an eminent physiologist maintains that in the medulla oblongata a small part exists which is the focus of vital power. Moreover, it is certain that the medulla oblongata has a great share in the respiratory movements. Therefore it seemed probable that the ablation of such an organ, even in cold-blooded animals, ought to be speedily followed by death. Such is not, however, the result of that operation; and in favorable conditions, the batrachia, for instance, can live more than four months after the loss of the medulla oblongata. During all that time, these animals, in appearance remain in good health, and I have observed in them the existence of all the following functions and properties:

* See the Bulletin de la Soc. Philomatique. Paris, 1849, p. 117.

1. The circulation of blood continues as well as in un-mutilated frogs. The beatings of the heart are at first quickened generally during half an hour, an hour or an hour and a half, after the operation; they then return to their normal rhythm, and they are found as regular and vigorous in frogs deprived of their medulla oblongata, for several days or even several months, as in healthy frogs. Sometimes, particularly when the hemorrhage has been considerable, the beatings of the heart become less numerous and less energetic; then the animal dies very quickly, but if it lives, the movements of the heart resume before long their normal rhythm and strength.

2. The pulsations of the four lymphatic hearts take place as in healthy frogs.

3. Digestion seems to be carried on as well and as quickly in frogs without medulla oblongata as in healthy frogs. I have ascertained this fact by introducing pieces of earth-worms into the stomach of these animals, and by studying the changes produced in these aliments during their passage along the digestive canal. Although very slow, chymous transformation, absorption and the production of fæces took place.

4. The products of the gastric, intestinal, biliary and pancreatic secretions being very useful, if not essential to digestion, it is very probable that these secretions exist.

5. The urinary secretion and also the production of cutaneous and intestinal epithelium, are performed as usual.

6. The pulmonary respiration ceases, but the cutaneous respiration is continued. The absorption of poisons by the skin and by the mucous membranes exists as in healthy frogs.

7. The reflex faculty is energetic, and so much so that the frogs deprived of the medulla oblongata can raise by a reflex action, greater weights than healthy frogs. As reflex movements exist, I need not say that muscles and nerves have kept their vital properties. It is frequently found that the spinal cord, especially in the *rana temporaria*, becomes so excitable that the slightest irritation of the skin is followed by tetanic convulsions.

8. The galvanic current of muscles not only exists in frogs deprived of medulla oblongata, but appears to be stronger.

From these facts it results clearly that frogs, deprived of

their medulla oblongata, are in full life. It is so much so that if they are compared to frogs possessing that nervous centre, they resist etherisation longer, and also live longer after the ablation of the heart.

II. The greatest differences exist in the duration of life, after the removal of the medulla oblongata, in animals of different species, as will be seen in the following table, where the maximum duration of life is indicated in sixty different species of animals:

Classes.	Species.	Duration of life.
Amphibia.	{ Salamanders	} More than 4 months.
	{ Frogs	
	{ Toads	
Reptilia.	{ Tortoises	} 9 to 10 days.
	{ Snakes	
	{ Lizards	
Fishes.	{ Eel	} 6 days.
	{ Pike, carp, tench, eel pout, barbel	
	{ Perch, gudgeon and others .	
Birds.	{ Sparrowhawk (newly born) .	} 21 minutes.
	{ Magpie do. .	
	{ Sparrow do. .	
	{ Sparrow, yellowhammer, linnet, pi- geon, fowl, duck, pintaw, par- tridge, moor-hen, turtle-dove (adult)	
	{	
Mammals.*	{ Dormouse (during hybernation)	} 29 hours.
	{ Hedgehog ditto	
	{ Bull-dog (newly born) . .	
	{ Cat ditto . .	
	{ Rabbit ditto . .	
	{ Guinea-pig ditto . .	
	{ Dormouse and Hedgehog awaken in summer	
	{ Cat, rabbit, guinea-pig and dog (adult)	

The preceding table shows that after the removal of the medulla oblongata, in different species of animals, the duration of life may be reckoned by *months* for batrachia, by *weeks* for some reptilia, by *days* for other reptilia and for fishes, by *hours* for

* Pulmonary insufflation has been used only for the dormouse and hedgehog during hybernation.

hibernating mammals, and by *minutes* for birds and non-hibernating mammals.

III. After the removal of the medulla oblongata, the most remarkable differences in the duration of life, in different individuals belonging to the same species, may occur in consequence of differences of temperature. The lower the temperature the longer is the duration of life. Thus, the duration of life in frogs may be reckoned by *months* when the temperature is between 32° and 46° F., (0 and 8° Cs.); by *weeks* when it is between 40° and 55° F. (5° and 13° Cs.); by *days* when it is between 50° and 65° F. (10° and 18° Cs.) by *hours* when it is between 65° and 72° F. (18° and 24° Cs.); and by *minutes* when it is between 86° and 105° F. (30° and 40° Cs.)

In the other cold-blooded vertebrata the differences in the duration of life, after the removal of the medulla oblongata, are not so great as in frogs, but the law is the same. This law exists also for warm-blooded animals, so much so that the differences existing between mammals of different ages and of different species, are to be attributed, in part, to their differences of temperature.

IV. As the principal condition for a long duration of life in cold-blooded vertebrata is a cold atmosphere, and as the vital phenomena taking place in these animals are much diminished when they are exposed to a low temperature, some physiologists have supposed that the persistence of life for many weeks or more, was equivalent, as regards the sum of the vital phenomena, to a duration of some hours in summer, when these phenomena have a great activity.

In answer to this objection I will at first call the reader's attention to the fact that, in batrachia, deprived of the medulla oblongata, and exposed to the action of a low temperature, the heart beating, on an average, 35 times in a minute and life lasting four months—*i. e.* 172,800 minutes, it follows that, during that time, the heart has more than 6,000,000 pulsations. In summer, the maximum duration of life having been six hours—*i. e.* 360 minutes, and the heart beating, on an average, 45 times in a minute, it results that, during that life, the heart has only 1,600 pulsations—a number which is to the other as 1 is to 375.

This comparison shows how incorrect it is to suppose that in consequence of the diminution of the vital phenomena in cold weather, a batrachian that lives several months does not live more than another living only several hours in summer.

This opinion is also proved erroneous by the facts which I have already related, and which show that all the functions and vital properties existing in frogs deprived of their medulla oblongata, appear to be as active as in unmutilated frogs. No doubt that there is a notable difference between summer and winter as to the activity of vital phenomena in frogs, but if we suppose that these phenomena are in winter only the tenth of what they are in summer, and if we consider, consequently, the duration of life in winter as being only the tenth of what it is, we shall have, nevertheless, a duration in winter forty or fifty times as great as the duration in summer.

V. Now I have to examine why, in summer, the life of cold-blooded vertebrata, deprived of the medulla oblongata, is much shorter than in winter.

The principal cause of this difference is in the fact that the cutaneous respiration, (it is known that the pulmonary respiration does not exist in animals deprived of the medulla oblongata,) which is sufficient as long as the temperature is very low, becomes more and more insufficient, when the temperature becomes more and more elevated. So that the same law exists for the cold-blooded vertebrata deprived of their medulla oblongata, and for those which are not mutilated. The following experiments concur to demonstrate the correctness of this view:

I have found that frogs deprived of their medulla oblongata live much longer when placed in oxygen than in atmospheric air. I have made two series of experiments—one in June, 1847, the other in July, 1850. In both series the frogs were put immediately after the operation, under a receiver full of oxygen. They lived from eight to fourteen days at a temperature at which life, in atmospheric air, is always shorter than six hours. The temperature was from 64° to 84° F. (18° to 29° Cs.) These frogs would have lived longer if the quantity of oxygen had been more considerable.

By pulmonary insufflation I have maintained life in tortoises deprived of the medulla oblongata, much longer than when in-

sufflation is not used. The duration of life was 7, 12, 13 and 17 days in four insufflated tortoises. When death occurred in these four animals, they had been left without insufflation more than five or six hours, and very likely they would have lived longer had they been insufflated more frequently. In four non-insufflated tortoises life lasted 3, 7, 19 and 23 hours. These comparative experiments were made in summer in a temperature varying from 64° to 86° F. (18° to 30° Cs.) The insufflated tortoises lived longer in summer than non-insufflated tortoises in winter.

From all these facts it is evident that in animals deprived of the medulla oblongata, death is principally caused by insufficiency of respiration.

XVII.—ON THE INFLUENCE OF THE TEMPERATURE OF A WARM-BLOODED ANIMAL UPON THE DURATION OF ITS LIFE WHEN IT IS ASPHYXIATED.

One of the most positive facts in physiology is that every animal needs oxygen in order to live. But if there is a complete uniformity as to the necessity of oxygen for all animals, there is also the greatest variety between the different species as to the quantity of that gas which is necessary for the maintenance of life. Very probably a great part of the physiological differences between different animals, comes from the difference in the quantity of oxygen absorbed by their blood in a given time. The most important cause of the differences existing between cold and warm-blooded animals, and between young and adult animals, is to be found in the differences in the quantity of oxygen they absorb.

It is very remarkable that one of the principal laws relative to the anatomical differences existing between the different species of animals, appears to be also a law regulating their physiological differences. In an anatomical point of view, a mammal, at the different periods of its development, presents alternately the forms of many different beings. In a physiological point of view, a corresponding transformation occurs for the mammals. They exhibit alternately the same phenomena of life exhibited by many other different animals. For instance, before its birth, the mode of breathing of a mammal is the

same as that of fishes. The oxygen absorbed by fishes exists in a liquid; the oxygen absorbed by the mammal foetus exists also in a liquid, which is the blood of the mother. In fishes and also in mammal foetuses, oxygen has to pass through many membranes: in fishes, through the mucous membrane of the bronchiæ and the membrane of the capillary vessels; in the mammal foetuses, through the membranes of the capillary vessels of the maternal and the foetal placentas, and their mucous coverings. After birth young mammals have an insufficient power of breathing, and in this they are like the reptilia. They are unable to absorb a sufficient quantity of oxygen to resist the influence of cold; and their power of retaining life when deprived of oxygen, is comparable to that of the reptilia.

This fact has been well established by the experiments of Buffon, Boyle, Ens, Roose, Haller, Fontana, Legallois, and more particularly W. F. Edwards. Nevertheless, these eminent experimenters have left many important questions without solution, some of which I will examine here.

In order to study the influence of temperature on the duration of life in asphyxiated animals, W. F. Edwards dipped into water, at different degrees of temperature, many animals of different ages. Unhappily he did not take notice of the temperature of the animals on which he experimented; and we will show that this circumstance is very important, because the degree of that temperature at the instant when asphyxia begins, has a considerable influence on the duration of life. Consequently, to discover with exactitude the influence of the temperature of a medium, on animals deprived of breathing, it was necessary to operate on animals at the same temperature. This has been done neither by Edwards nor by any other physiologist.

A priori it is easy to acknowledge that the duration of life in animals asphyxiated can be influenced by four capital circumstances: 1st. The degree of the temperature of the animals; 2d. The degree of the temperature of the medium; 3d. The age of the animals; 4th. Their species.

Consequently four series of experiments were to be performed. I have made them, and I propose to give here some of the results I have obtained. The remaining shall be detailed in a special paper.

I. *Influence of the Temperature of young warm-blooded animals on the length of their resistance to Asphyxia.*

Experiment 1. Nine rabbits of the same brood and aged about two days, were dipped into water at 25° Cent. (77° Fahr.) Two of these animals, No. 1 and No. 2, had the temperature which they generally have when they are in their nest covered by their mother. The others had been cooled by having been exposed to the action of an atmosphere at 10° Cent. (50° Fahr.) No. 3 and No. 4 had been exposed for a quarter of an hour; No. 5 and No. 6 for three quarters of an hour; and No. 7, No. 8 and No. 9 for an hour and a half. The results are represented in the following table:

TABLE I.*

Nos.	Temperature of Animals.		Duration of Life.	Mean Duration of Life.
1,	35° to 36° Cent.	(95 to 97° Fahr.)	10'	12'
2,	ditto	ditto	14'	
3,	29° to 30° Cent.	(84° to 86° Fahr.)	16'	18½'
4,	ditto	ditto	21'	
5,	23° to 24° Cent.	(74° to 75° Fahr.)	20'	23'
6,	ditto	ditto	22'	
7,	18° to 19° Cent.	(65° to 66° Fahr.)	22'	28'
8,	ditto	ditto	29'	
9,	ditto	ditto	33'	

The one that lived shortest (10'), was nearly at 36° Cent. (97° Fahr.) ; the one that lived the longest (33'), was nearly at 18° Cent. (65° Fahr.) The difference of temperature being 17° or 18° Cent. (about 32° Fahr.) the difference as to the duration of life was 23 minutes—that is, nearly two minutes for each diminution of three degrees Fahr.

The same experiment performed on many breeds of rabbits, has always given very nearly the same results.

These experiments prove that the temperature of young rabbits has a decided influence upon the duration of their life, when they are deprived of breathing. I have ascertained that the same thing takes place in cats, dogs, mice, and many species of birds.

Experiment 2. I dipped into water at the temperature of 25° Cent. (77° Fahr.) three bull-dogs.

* The existence of reflex movements has been used in this experiment and in all the others as the proof of life.

TABLE II.

Nos.	Temperature of Animals.				Duration of Life.
1,	.	38° Cent.	(about 101° Fahr.)	.	15'
2,	.	30	" 86 "	.	24'
3,	.	22	" 77 "	.	47'

Between No. 1 and No. 3 the difference of temperature was 16° Cent. (29° Fahr. ;) the difference in the duration of life was thirty-two minutes—that is, two minutes for each diminution of one centigrade degree (nearly 2° Fahr.)

Experiment 3. I put a ligature around the trachea of four cur-dogs of the same brood, and aged three days.

TABLE III.

Nos.	Temperature of Animals.				Duration of Life.
1,	.	37° Cent.	(about 99° Fahr.)	.	13'
2,	.	28	" 83 "	.	19'
3,	.	24	" 76 "	.	31'
4,	.	19	" 67 "	.	51'

So that between the two extremes the difference as the duration of life, was 38 for 18 Cent. (about 32° Fahr.)

Many other experiments on very young dogs gave very nearly the same results.

Experiment 4. On five cats, of the same brood, and aged two days, I put a ligature around the trachea, after having cooled three of them.

TABLE IV.

Nos.	Temperature of the animals.				Duration of life.	Mean duration.
1,	36° Cents.,	(about 97° Fahr.)			21' }	25½'
2,	36	" " 97	"		30 }	
3,	23 to 24 Cent.,	(74 to 76 Fahr.)			47 }	48½
4,	22 to 23 Cent.,	(72 to 75 Fahr.)			50 }	
5,	17 Cent.,	(about 63 Fahr.)				53

The temperature of the air was then at 18½° Cents., (65 Fahr.)

Experiment 5. On four mice, probably aged from three to eight days, I put a ligature around the trachea after having cooled three of them.

TABLE V.

Nos.	Temperature of the animals.				Duration of life.
1,	34° Cents.,	(about 94° Fahr.)			11'
2,	27	" " 81	"		14
3,	22	" " 72	"		10
4,	18	" " 65	"		14

I have obtained analogous results in experimenting on birds.

Experiment 6. On seven magpies, aged from four to eight days, I put a ligature around the trachea, and opened widely the thoraco-abdominal cavity.

TABLE IV.

Nos.	Temperature of the animals.					Duration of life.	Mean duration.
1,	36° Cent., (about 97° Fahr.)					20'	24,
2,	36	"	"	97	"	28	
3,	32	"	"	97	"	26	32½
4,	31½	"	"	90	"	39	
5,	26	"	"	79	"	12	50
6,	26	"	"	79	"	58	
7,	22	"	"	72	"		72
8,	20	"	"	68	"	65	76
9,	20	"	"	68	"	87	
10,	18	"	"	65	"	82	92
11,	10	"	"	65	"	103	

Experiment 7. On other magpies I made the same experiment, but without opening the thoraco-abdominal cavity, so that asphyxia was much more complete.

TABLE VII.

Nos.	Temperature of the animals.				Duration of life.
1,	35° Cent., (95° Fahr.)				8'
2,	30	"	86	"	15
3,	24	"	76	"	27
4,	19	"	67	"	39

I have obtained nearly the same results from experiments upon many sparrow-hawks, ravens and jays.

All these facts show how considerable is the influence of the temperature of certain young animals and birds on the duration of their life when they are asphyxiated.

II. *Differences in the length of the resistance to asphyxia according to the species of animals.*

In order to discover what is the influence of the species, I made the following experiment. I tied the trachea on twenty-two animals belonging to eleven species, all of which were aged about four or five days. Their temperature had been cooled and they were all at about 26° Cent., (79° Fahr.) The temperature of the air was 19° Cent. (67° Fahr.)

TABLE VIII.

Nos.	Species.	Duration of life.	Mean duration of life.
1,	Rabbit	18'	21'
2,	do.	24	
3,	Guinea pig	6	
4,	do.	9	7½
5,	Mouse	20	
6,	do.	27	
7,	Dog	25	32
8,	do.	39	
9,	Cat	31	
10,	do.	36	33½
11,	Sparrow	8	
12,	do.	12	
13,	Pigeon	5	7
14,	do.	9	
15,	Jay	13	
16,	do.	17	15
17,	Raven	14	
18,	do.	23	
19,	Sparrow Hawks	16	18½
20,	do.	24	
21,	Mag-pie	18	
22,	do.	25	22½

Now if we compare birds to mammals, we see that generally mammals resist asphyxia more than birds. The two following tables show the difference :

TABLE IX.

Mammals.

Guinea-pigs	7½'
Rabbits	21
Mice	23½
Dogs	32
Cats	33½
Average	23½

TABLE X.

Birds.

Pigeons	7'
Sparrows	10
Jays	15
Raven	18½
Sparrow Hawks	20
Mag-pies	21½
Average	15½

III. *Influence of the temperature of adult warm-blooded animals on the duration of life when they are asphyxiated.*

I have discovered that the degree of the temperature of adult non-hybernating vertebrata has also a great influence on their

power of resisting asphyxia. Dogs, cats, rabbits, guinea-pigs and birds, are subject to the same law when they are adult as when they are very young. The lower their temperature, the longer they live when they are asphyxiated. But although the existence of this law for adult vertebrata is beyond all doubt, it is sometimes very difficult to ascertain that existence. It is not easy to diminish the temperature of a non-hibernating adult mammal, or that of a bird, without exhausting the nervous and the muscular power of the animal, and also without producing a general and considerable perturbation. The action of a cold bath, for instance, so powerfully excites the vertebrata, that sometimes violent convulsions take place, and then death may occur in a short time, even before the animal has lost 10° cent. (18° Fahr.) of its temperature. However, some individuals may be cooled without being exhausted by convulsions. I have experimented on a great many which were in this condition. For more than eight years, without intending to make experiments on this subject, I have had the opportunity of stating, on more than a hundred adult rabbits or guinea-pigs, used for other researches, that, when their temperature is diminished, the duration of their life, when they are completely deprived of breathing, is decidedly longer than in the normal state. Whatever has been the cause of the cooling, the effect has been the same. In these numerous experiments, the animal heat has been diminished, either by a disease, by a poison, by an injury in the nervous centres, by the immersion of the animal in melted ice, or by the application of a layer of oil, essence, or gelatine, to the whole skin of the animal.

The best mode of cooling a non-hibernating warm-blooded animal is to make the following experiment, in a room where the temperature of the air is not far from freezing point. I remove the superior part of the cranium of a mammal or of a bird, and afterwards cut the brain, slice by slice, from the anterior to the posterior extremity. After the ablation of the cerebrum and the cerebellum, the animal is put on the floor, where it is left perfectly quiet for an hour or two. If the experiment is made on a rabbit or a guinea-pig, the temperature of the animal then falls from 40° Cent. (104° Fahr.) to 30° or 35° Cent. (86 or 95° F.)

A ligature being put around the trachea, I find that the animal is then able to live generally from six to 8 or 12 minutes.

The more the animal heat is diminished, the more, in general, is the resistance to asphyxia, except in cases where cooling has been produced too quickly.

On four adult rabbits, I put a ligature around the trachea, and have obtained the following results :

TABLE XI.		
Nos.	Temperature of the animals.	Duration of life.
1,	39½° Cents., (103° Fahr.)	3½'
2,	35 " 95 "	6
3,	30½ (between 86 and 87 Fahr.)	10
4,	25 Cents., (77 Fahr.)	14

On three guinea-pigs I put a ligature around the trachea, and found the following facts :

TABLE XII.		
Nos.	Temperature of the animals.	Duration of life.
1,	40° Cents., (104° Fahr.)	2¾'
2,	35 " 95 "	5½'
3,	30 " 86 "	12

The longest persistence of life, after the cessation of breathing which I have found in adult non-hibernating animals, has been in a cat aged five months, and whose temperature had been diminished to 19° Cent. (about 67° Fahr.) in consequence of the laying bare of the abdominal viscera, and of the ablation of the cerebrum by small parts.

I will say only a few words about my experiments on adult birds.

Two pigeons were dipped into melting ice. In one of them, whose temperature had fallen from 42° Cent. (108° Fahr.) to 35° Cent. (95° Fahr.) life lasted six minutes after the ligature of the trachea. In the other, life lasted nine minutes; its temperature had fallen from 42° Cent. (108° Fahr.) to 30° Cent. (86° Fahr.)

I have obtained nearly the same results in experimenting on fowls and on ducks.

Physicians are generally astonished at seeing that men attacked with cholera are able to live almost without breathing. My experiments show that the principal, if not the only cause

of the power living with so deficient a respiration, is the diminution of temperature. The same thing occurs in the last hour of life in many cases of diseases, and particularly those of the brain, of the respiratory organs, and in that dreadful disease of children called *scleroma*.

It is easy to understand how a considerable breathing becomes less and less necessary when the temperature of the animals under experiment is diminishing.

Whatever may be the function of oxygen, it is positive that most of the chemical changes which take place in the living animals are accompanied with a consumption of oxygen. If so, when these chemical changes are diminished, the consumption of oxygen ought to diminish. Now, as every diminution of the temperature of an animal produces a proportionate diminution in nearly all the acts of organic and animal life, and as these acts are all accompanied by chemical changes in which oxygen is consumed, it follows that, when they diminish, the consumption of oxygen also diminishes.

In the act of running, or in a rapid walk, we consume much more oxygen than in a state of rest. What we call rest is merely a state of diminished activity; and when the temperature of a mammal has been cooled down, the activity of most of the functions is much more diminished than in the most complete rest. It results from this fact, that when a warm blooded animal has lost a notable part of its ordinary warmth, its consumption of oxygen is much less than in a state of rest.

If the ligature of the trachea is made simultaneously on two adult mammals of the same species, one of them being at its normal temperature, and the other at a temperature of 5° Cent. (9° Fahr.) lower, we observe that this last one lives six, seven or eight minutes, and the other from one and a half to three minutes. If we suppose the quantity of oxygen existing in the lungs and in the blood of these two animals is precisely alike in both; and if we admit that death occurs in asphyxia, either from want of oxygen or by an action of carbonic acid, in both cases these two animals ought to live a different length of time, because the consumption of oxygen contained in blood and the production of carbonic acid are quicker in one of them than in the other. This explanation is so true, that when movements are

excited in the cooled animal, after the beginning of asphyxia, it dies sooner than when it remains in rest.

From the facts and reasonings which are related in this note I draw the following conclusions :

1st. The temperature of newly-born warm-blooded animals has a great influence on the duration of their life when they are asphyxiated.

2d. There are very considerable differences in the duration of life in asphyxiated animals of different species, even when the experiment is performed in the same medium, and while their temperature is at the same degree.

3. The degree of the temperature of adult warm-blooded animals has also a great influence on the duration of their life when they are asphyxiated.

4th. The influence of the animation of the temperature of a warm-blooded animal on the duration of its life in asphyxia, explains the persistence of life in man, in cases of cholera, scleroma, and of some other diseases, when respiration is much diminished.

XVIII.—ON THE CENTRAL SEAT OF GENERAL AND OF TACTILE SENSIBILITY, AND ON THE VALUE OF CRIES AS MANIFESTATIONS OF PAIN.*

To determine where is the seat of perception and of volition is one of the greatest physiological questions. Flourens maintains that this seat is in the central lobes. Many physiologists, among whom are Bouillaud, Gerdy and Longet, have published papers against the doctrine of Flourens. Their only important argument is that mammals deprived of their whole encephalon, except the medulla oblongata and the pons varolii, continue to possess the faculty of perceiving sensations, and that the perception of pain is then manifested by cries and agitation. When the encephalon, says Longet,† is so much mutilated, in rabbits and dogs, that only the pons varolii and the medulla oblongata remain, in the cranial cavity, these animals, although they seem to be in a deep coma, are still able to agitate themselves, and to cry plaintively, under the influence of strong external irritations; but if a sufficiently deep alteration is made

* See *Comptes Rendus de l'Acad. des Sciences*, 1849. t. xxix. p. 672.

† *Traite de Physiol.* Paris. 1850. t. ii, B. p. 38.

in the pons varolii there is an immediate cessation of the cries and of the agitation; it merely remains an animal in whom the circulation, the respiration and the other nutritive functions are momentarily accomplished.

On cats, rabbits and guinea-pigs, I have obtained a completely different result in performing that experiment. After I had removed, slice after slice, and from forwards backwards, the whole encephalon, except only the medulla oblongata, I have found that the mutilated animal, not only is much agitated, but cries plaintively when it is pinched. If the medulla oblongata is also removed, the cries cease, but the agitation still continues.

According to these experiments, it is evident that the pons varolii is not the only *seat of sensibility, the centre of perception of tactile impressions*, as Longet calls it, and that either the medulla oblongata is the seat of general sensibility or the cries do not prove that there is a perception.

Longet considers that the pons varolii is not only the centre of perception for sensation of pain, but also for tactile impressions. As to the tactile sensations he does not give the slightest appearance of proof. Certainly neither the cries nor the agitation are sufficient to authorize the opinion that the animal has felt a sensation of tact. If the existence of cries could prove that there is a perception of a tactile sensation, I should have to conclude from my experiments that the medulla oblongata is the seat of the faculty of perception of tactile sensations, for there are cries after the removal of the whole encephalon except the medulla oblongata, and there are no more after the removal of this last organ. If, instead of drawing conclusions from the existence of cries, we take notice only of the agitation, we are bound to conclude that the spinal cord is the seat of the faculty of perception of tactile sensations, for after this organ has been separated from the medulla oblongata, agitation takes place when a limb is pinched.*

Cries and agitation may be attributed to a property of the nervous centres, which is completely different from the faculty of perception of painful or tactile sensations. That property is the reflex faculty of the true spinal marrow;† it is the property of

* This conclusion is maintained as true by Sénac, Caldani, Kay, Legallois, Paton, J. W. Arnold and many others.

† As Dr. Marshall Hall calls it.

uniting, in co-ordinate movements isolated muscular contractions,* which is called by German physiologists the faculty of adaptation to an end. That property manifests itself by movements similar to those executed by unmutilated animals when they feel a pain; and it happens, sometimes, that these reflex movements are less disordered than the movements consecutive to a violent pain in an unmutilated animal. The *agitation* of the animals deprived of all the parts of their encephalon is merely the result of an action of the reflex faculty.

The cries also appear to exist only in consequence of a reflex action. This appears to be difficult to be proved, and it will seem nearly impossible to admit that cries may be produced by an animal that has felt no pain, or that has not had the will of crying. We may consider a cry as a noise produced in the larynx, as many times a quick expiration is performed when the vocal cords are stretched. Now as the tension of these cords and the expiration is produced by muscular contractions, it is easy to understand that these contractions are produced by a reflex action as well as the contractions of the muscles of the limbs.

For those who know that hiccup, coughing, sneezing, vomiting and so forth, frequently are mere reflex phenomena, there ought to be no difficulty in admitting that crying is a pure reflex action.

If we here use the expressive language of Flourens, we will say, that the medulla oblongata has the faculty of uniting in a co-ordinate movement, the contractions of the expiratory muscles and that of the tensor muscles of the glottis.

From the facts and reasonings contained in this note I will now draw the following conclusions:

1st, That the experiment by which many physiologists have endeavored to prove that the cerebral lobes are not the exclusive seat of the perceptions, do not give such a proof.

2d, That animals can cry after the removal of the whole encephalon, except only the medulla oblongata.

3d, That the existence of cries cannot prove that there is a perception of pain, because cries result from muscular contractions which may be pure reflex actions.

* That is the name given by Flourens more than 25 years ago.

4th, That there is no proof that the pons varolii is the centre of perceptions either of touch or of pain.

5th, That if it is admitted that cries prove that there is a perception of pain, we should have to admit that the medulla oblongata is also a centre for these perceptions.

XIX.—ON THE MODE OF ACTION OF SOME OF THE MOST ACTIVE POISONS UPON THE NERVOUS SYSTEM.

Some of the results of my experiments on this subject have already been published in the inaugural dissertation of my learned friend and pupil, Dr. F. W. Bonnefin, with whom I performed most of these experiments.*

I intend to give here only the results of my researches on the poisons which produce convulsions. For the sake of brevity I have called them *convulsing* poisons. The most important among them are: strychnine, brucine, picrotoxine, morphine, digitaline, cyanhydric acid, nicotine, cyanide of mercury, sulphide of carbon, chloride of barium, and oxalic acid.

The principal questions which I have endeavored to solve are the following:

1. What is the part of the animal frame upon which these poisons act in producing convulsions?

2. By what mode of action do they produce convulsions?

The parts of the body on which they could act are:—

- a. The nervous centres.
- b. The motor or centrifugal nerves.
- c. The sensitive or centripetal nerves.
- d. The muscles.

Of course, the convulsing poisons could act upon these four parts together, or upon two or three of them.

Now, as to the mode of action, these poisons could produce convulsions *directly* or *indirectly*.

In other words these poisons might act:—1. As *excitants* either of the muscles or of the nervous system, and this is what I call a direct action. 2. As causes of increase of the vital powers of the muscles or of the nervous system, so that even a slight excitation of these parts is able to produce convulsions, and this is what I call an indirect action.

* Rech. Expériment. sur l'action convulsivante des principaux Poisons, in 4 Paris, 1851.

The convulsing poisons do not appear to excite the muscles or any part of the nervous system, and consequently they do not act directly. They act merely in increasing the vital powers of the nervous system, so that a slight excitation of the skin or another part where there exist nerves of sensation is sufficient to produce convulsions. Therefore these poisons act indirectly in the production of convulsions. They do not produce convulsions, they merely act upon the nervous system, so as to render it capable of producing convulsions when it receives an excitation. They do not excite the nervous system, and their mode of action is altogether different from that of the mechanical, physical, and chemical excitations which directly produce convulsions, when they are applied to the medulla oblongata, or to the spinal cord.

This mode of action was already known as regards strychnine. Long ago Mr. Magendie* found that animals poisoned by *nux vomica*, frequently remain without convulsions as long as they are left without excitation, but that fits are immediately produced when they are touched. Since that time Stannius,† Van Deen,‡ Pickford,|| J. W. Arnold,§ Meyer, of Zurich,¶ Marshall Hall,** and A. Barnard,†† have observed many facts proving that strychnine does not directly produce convulsions. The following experiment, which I performed with the assistance of Dr. Bonnefin, is still more demonstrative.

I introduce a large dose of strychnine into the stomach of a frog, after the removal of its brain and of its medulla oblongata. As the voluntary and the respiratory movements are then impossible, there is no spontaneous movement at all, and if the animal is left perfectly quiet there are no convulsions. But as soon as it is excited, even by the slightest touch, tetanus occurs.

In this case, it is evident that the convulsive fit is merely a re-

* Examen de l'action de quelques végétaux sur la Moelle épinière. Paris 1809, p. 7.

† Mueller's Archiv. 1837, p. 223.

‡ Traité et découvertes sur la Moelle épinière, 1841, p. 123.

|| In Rorer und Urardtich's Vierteljahreschrift. Bd. 2, 1843, p. 430.

§ Ueber die verrichtung der Wurzeln der Rückenmarksnerven, 1844.

¶ Schmidt's Jahrbücher, 1847, No. 8.

** Comptes rendus de l'Acad. des Sciences, 1847, vol. 24, p. 1054.

†† Procès verbaux de la Société Philomatique, 1847, p. 71.

flex act. But why is there a tetanic contraction instead of the regular reflex movements? The reason is that the reflex faculty is considerably increased. This will be proved in a moment.

Before giving this demonstration, I must examine if there is not also an increase in the vital powers of the muscles and of the motor nerves. There was no reason to reject the supposition that the convulsing poisons were capable of increasing simultaneously the vital powers of the nervous centres, of the nerves, and of the muscles. Consequently, I was led to perform the following experiments, which prove:—1st, that these poisons do not increase the vital powers of the motor nerves and of muscles; 2d, that they do not act as direct excitants upon these organs.

On young cats, on birds, and on reptiles, I removed the whole portion of the spinal cord which supplies nerves to the posterior limbs. A few minutes after, I injected into the rectum a solution of a salt of strychnine. Convulsions occurred only in the anterior parts of the body, and when I excited either the motor nerves or the muscles of the posterior limbs, contractions were produced exactly as in animals not poisoned, but there was no appearance of convulsions.

When the poison was placed directly on muscles or on nerves, or when it was injected with blood in a limb separated from the body, there was no appearance either of an excitation, or of an increase in the excitability of the muscles, or of the motor nerves.

The experiments of Magendie, of Emmert,* and of Backer,† have demonstrated that after a transverse section of the spinal cord, between its two enlargements, convulsions may be produced in the palsied limbs, when the animal is poisoned with strychnine. All the physiologists who have performed this experiment have found it perfectly exact. Nevertheless, J. W. Arnold,‡ maintains that the action is much less considerable in the posterior limbs, in that case, than when the spinal cord is uninjured and united with the medulla oblongata, and he concludes that the poison

* *Exper. de effectu venenorum veget. americ. in corpus animale*, 1817.

† *Commentatio ad questionem physiol., ab Acad. Rheno traject, anno 1828, propositam*.

‡ *Die lehre von der Reflex-function*. Chap. ix. and x. 1842.

acts much more on the medulla oblongata than on the spinal cord.

Sometimes, as Arnold says, it occurs that the action of strychnine, in mammalia and in amphibia, is not so powerful in the palsied parts, after the section of the spinal cord, as in the non-palsied limbs. As to mammalia, the reason of this difference is the diminution in the quantity of blood received by the part of the spinal marrow separated from the encephalon. As to amphibia, it is easily seen that immediately after the section of their spinal cord, the reflex faculty in the part separated from the encephalon is very weak, and if, then, strychnine is given to the animal, it does not act very strongly; but if the poison is given two or three hours after the section of the spinal cord, then the reflex faculty is very powerful and the poison acts violently, and sometimes more than if the spinal cord was uninjured. In birds, which, as I have discovered, have constantly a powerful reflex faculty after the section of the spinal cord, strychnine acts very energetically.

Lately, Stannius and Cl. Bernard have supposed that strychnine, instead of acting on the spinal marrow, acted on the nerves of sensibility, and more particularly on their termination in the skin. They base this hypothesis on some experiments, of which only one is important. After the section of the spinal cord, at the brachial enlargement, upon a frog, the animal is poisoned with strychnine, and then convulsions occur nearly at each voluntary or respiratory movement. But if the sensitive roots of the spinal nerves are cut, the convulsions cease immediately.

Now, it is evident that this fact does not prove what Stannius and Bernard have supposed; because it may be explained as well by admitting that convulsions are produced only in consequence of an increase of the reflex faculty of the spinal cord, as by the hypothesis of Stannius.

Van Deen* relates an experiment which is in opposition to the theory of Stannius. If we take a frog prepared exactly as in the experiment of this physiologist, we see that tetanus occurs when the animal is thrown on the floor. In this case tetanus is produced, although the sensitive roots are cut and unable to act; therefore tetanus is a consequence of an increased

* Loco cit. p. 123.

vitality in the spinal cord itself. But this experiment is not decisive, because sometimes it occurs that such a mechanical excitation in frogs, which have not been poisoned, produces tetanus. Nevertheless I must say that the tetanus in this last case is never so violent as in poisoned frogs.

A better experiment is to excite slightly with a needle, the posterior columns of the spinal cord. Then, tetanus constantly occurs in poisoned frogs, although the sensitive roots are divided, and it very rarely occurs in frogs that are not poisoned.

I have already published* the following experiments, which are much more decisive against the hypothesis of Stannius:

A ligature is put around the aorta of a frog, near its termination in the abdomen, and consequently the posterior limbs cease to receive blood. Then the frog is poisoned with strychnine introduced into its mouth, and after a few minutes the convulsive phenomena take place.

In this experiment the nerves of the posterior limbs do not receive strychnine, whilst the spinal cord receives it; therefore convulsions are not produced in consequence of an action of strychnine on the sensitive nerves of the skin, as Stannius has supposed, but in consequence of its action on the spinal cord.

Now, if we poison a frog after having divided the spinal cord at the brachial enlargement, and after the section of all the small arteries giving blood to the spinal column, we see that convulsions do not take place in the posterior limbs, although the reflex faculty is not lost in consequence of the cessation of the circulation in the spinal cord, and that it remains for half an hour or a little more in summer, and about two hours in winter.

In this experiment, blood containing strychnine reaches the sensitive nerves of the posterior limbs, and not the spinal cord, and there are no convulsions; therefore it is not on the sensitive nerves that strychnine acts in order to produce convulsions.

These two experiments are evidently decisive. In the first, we see that when blood containing strychnine reaches the spinal cord, and not the cutaneous nerves, there are convulsions; and in the second, we see that when blood containing strychnine reaches the sensitive nerves and not the spinal cord, there

* *Gaz. Med. de Paris*, 1849, p. 745.

are no convulsions. We must consequently draw from these experiments these two conclusions:

1. Strychnine does not act upon the sensitive nerves.
2. Strychnine acts upon the spinal cord.

Now, from all the facts above related, two other conclusions are to be drawn:

1. Strychnine does not excite the nervous system; or, in other words, strychnine does not produce convulsions directly.
2. Strychnine increases the reflex faculty of the spinal cord, and so produces convulsions indirectly.

I do not intend to examine here whether strychnine kills in producing convulsions, or by another action. Nevertheless, I will say, that although convulsions are sufficient to kill in asphyxiating, death, in cases of poisoning by strychnine, may be also produced by another action of that poison. I have seen animals in which convulsions did not take place at all, and which have been killed by strychnine.

The other convulsing poisons that I have studied, appear to act as strychnine, as to the production of convulsions.

The same experiments which I have related as regards strychnine, have been performed with these poisons, and I have obtained the same results. Sometimes, nevertheless, I found some differences; and, for instance, it appears that the chloride of barium is a direct exciter of the muscular fibres, and cyanhydric and oxalic acids seem also to be slight but direct exciters of the spinal cord.

The action of the chloride of barium is very important, because that poison is an exciter of the muscular fibres, and not of the nerves. This fact proves that the muscular irritability may be put in action without the intervention of the nerves.

The increase of the reflex faculty, by the convulsing poisons, is a very important fact. How is that increase produced? We believe it takes place in consequence of an increase in the nutrition of the nervous centres. J. Mueller* is of opinion that there is no substance able to increase directly the vital properties of any organ. He says that nutrition alone is able to produce such an effect. I believe he is perfectly right, and I admit that the mode of action of the convulsing poisons, in the

* Manuel de Physiol., edited by Littré, 1851, t. i. p. 582.

production of convulsions, is merely to increase nutrition in the nervous centres. It is important for practitioners to know that mode of action. The usefulness of strychnine in many cases of palsy, may be explained very easily by that action. I have frequently seen, in the wards of the hospital *la Charité* at Paris, paralytics under the care of Dr. Rayer, taking strychnine. Every day the reflex faculty was increasing in them as long as they took that substance; and on the contrary, when the use of that medicament was stopped, the reflex faculty began immediately to diminish, and in some patients it disappeared. If strychnine was given anew, the reflex faculty was still increased. These facts have been recorded with great care by my learned friend, Mr. Chareot. I hope he will publish them.

From all the facts narrated in this paper, I believe I am entitled to draw the following conclusions:

1. The convulsing poisons, more particularly strychnine, brucine, picrotoxine, cyanhydric acid, nicotine, morphine, cyanide of mercury, sulphide of carbon, digitaline, oxalic acid, appear to produce convulsions, without acting either directly or indirectly on the muscles or on the motor or sensitive nerves.

2. Generally these poisons do not appear to produce convulsions in acting directly on any part of the nervous centres.

3. These poisons, in producing convulsions, act only on the parts of the nervous system endowed with the reflex faculty.

4. The mode of action of these poisons consists in the increase of the nutrition of the nervous centres, by which excess of nutrition the reflex faculty becomes much increased.

XX.—ON THE CROSSED TRANSMISSION OF IMPRESSIONS IN THE SPINAL CORD.

Numerous experiments which I have performed have proved to the numerous physicians and students, who have seen the most important of them, that the impressions made on one side of the body are transmitted to the sensorium by the opposite side of the spinal cord.

It is known that Galen* performed two experiments, which

*See: *De locis affectis*, lib. iii. cap. xiv; or *De anatomicis admonstrationibus*, lib. viii. sect. 6.

have been considered as demonstrating that there is no *crossed* action in the spinal cord.

One of these experiments of Galen consisted in the transversal section of a lateral half of the spinal marrow. After this operation the animal was paralyzed in all the parts situated behind the section, on the same side, so that the palsy was on the right side of the body when the right side of the spinal cord was divided, and *vice versa*.

The second experiment consisted in a longitudinal section on the middle line of the spinal cord so as to separate into two lateral halves the part of that nervous centre supplying nerves to the posterior limbs. After this operation the animal was able to walk.

Galen, in these two experiments did not examine the state of the sensibility. He speaks merely of the voluntary movements. Nevertheless his researches were considered in this century as completely proving that there is no crossing of action in the spinal cord, either for sensibility or for voluntary movement.

The following experiments will prove that there is a crossing of action for sensibility in that organ :

1st. If a lateral half (*i. e.* the posterior and the antero-lateral columns and the gray matter of one side of the spinal cord), is divided transversely at the level of the tenth costal vertebra, on a mammal, it is soon evident that the sensibility is much diminished in the posterior limb opposite to the side of the sections. On the contrary the sensibility instead of being lost appears much increased in the posterior limb on the side where the section has been made.

2d. If, instead of one transversal section of the spinal cord, two, three, four or many more are made on the same lateral half of that organ, the same results are obtained.

3d. If, instead of mere sections, a removal of a part of a lateral half of the spinal cord, is effected, the same results are still obtained. In performing this experiment a longitudinal section, one inch in length, from behind forward, is made in the median plane of the spinal marrow, and then two transversal sections on a lateral half are made at the extremities of the longitudinal section, so that a part of the cord is completely separated from that organ and afterwards removed.

4th. If instead of dividing entirely a lateral half of the spinal cord, a small part is left undivided towards the centre of that organ, the posterior limb on the same side becomes much more sensible, but the posterior limb on the opposite side remains very sensible and sometimes it appears more sensible than in the normal state.

5th. If in performing the section of a lateral half of the spinal cord the instrument goes a little too far and divides also a small portion of the other half, then the posterior limb on the side of the complete section is less sensible than in the normal state, and the posterior limb of the opposite side, loses completely its sensibility.

6th. If the section of a lateral half of the spinal cord is made at the level of the second or third cervical vertebra, it is found that the sensibility becomes very quickly much greater in the parts of the body on the same side as the section, and on the contrary the parts on the other side becomes evidently less sensible.

7th. If after a section of a lateral half of the spinal cord at the level of the eleventh costal vertebra, another section is performed on the other side of that organ, at the level of the sixth costal vertebra, so that the two lateral halves are divided, then sensibility in most of the cases is lost, on both sides. Sometimes it retains a very slight degree of sensibility, more particularly in the posterior limb on the side where the spinal cord has been divided at the level of the sixth costal vertebra.

8th. If two sections of lateral halves are made as in the preceding experiment, but at a greater distance, one from the other, on the right side for instance at the level of the twelfth costal vertebra, and on the left side in the cervical region, nearly the same results are obtained as to the posterior limbs, but the sensibility is increased in the right anterior limb and it remains very evidently, but much diminished, in the left anterior limb.

9th. If a longitudinal section is made on the part of the spinal cord giving nerves to the posterior extremity, so as to divide that part into two lateral halves, then it is found that sensibility is completely lost in the two posterior limbs, although voluntary movements take place in them.

10th. If a similar separation of two lateral halves of the spi-

nal cord is made on the whole part supplying nerves to the anterior limbs, then it is found that sensibility is lost in both these limbs, and that it is only slightly diminished in the posterior limbs.

11th. If the same operation is done as in the preceding experiment, and afterwards if a transversal division is made on one of the lateral halves, in the place where it is separated from the other, then it is found that the posterior limb on the side of the transversal section remains sensible, and that the other posterior limb loses its sensibility.

These experiments prove very clearly that the sensitive nervous fibres are crossed in the spinal cord. The 9th, 10th, and 11th, demonstrate directly the crossing. In these experiments the crossed fibres are all cut, and sensibility is lost. This fact appears to prove that all the sensitive fibres cross each other; but it will be easily understood that on account of the loss of blood, and of the general diminution of sensibility produced by the excessive pain of the operation, if there are some fibres which remain without crossing, they are insufficient to give sensations.

As to the experiments consisting in transversal sections of a lateral half, they prove that sensibility is much diminished in the side of the body opposite to that of the section; consequently they prove also that there is a crossing of a great part of the sensitive fibres.

The fact that transmission of impressions made on one side of the body takes place, at least for a great part, in the opposite side of the spinal cord, is proved evidently by the eight first experiments, but much more by the 7th and the 8th experiments in which it is found that, after a section of a lateral half of the spinal cord, sensibility remains on the same side, and that it is nearly entirely lost after a second section of the other lateral half in another place.

If most of the nervous sensitive fibres are crossed in the spinal cord, then it is not exact to admit that the crossed paralysis of sensibility in cases of diseases of the brain, is explained by the crossing of fibres which exists in the pons Varolii and in other parts of the encephalon. Many opinions have been proposed as regards the place where the sensitive nervous fibres make their crossing in the encephalon. According to some pa-

thologists, this crossing takes place all along the medulla oblongata, the pons Varolii, tubercula quadrigemina and the crura cerebri. In all these organs there is truly a crossing of fibres, but we do not know what are these fibres. Ch. Bell believes that the crossing of the sensitive fibres takes place in the posterior surface of the medulla oblongata, in a great part of the length of the fourth ventricle. Longet supposes that this crossing exists at the anterior border of the pons Varolii, where the two *processi cerebelli ad testes* cross each other.

My experiments prove that if there are some fibres coming from the trunk and from the limbs which do not effect their crossing in the spinal cord itself, their number ought to be very small. Therefore the fibres which are found crossed in the encephalon are not sensitive fibres coming from the limbs and from the trunk, as all physiologists have supposed they were.

My experiments were made on many different species: guinea-pigs, dogs, cats, sheep, and rabbits. In all the same results were obtained.

To ascertain the degree of sensibility, I used various modes of excitation; mechanical, galvanic, physical, (*i. e.* warmth and cold,) and chemical. I constantly compared the degrees of sensibility in the parts of the body situated behind the injured portion of the spinal cord, with the anterior parts of the body, and particularly with the face. It is thus that I have been able to ascertain the existence of an increase or of a diminution in sensibility.

Sometimes I have given chloroform to animals having had a lateral half of the spinal cord divided in the cervical region. I have found that complete loss of sensibility appeared at first in the parts of the body opposite to the section of the spinal cord, the head and neck, and at last in the parts of the body behind the section of the cord, on the same side. This experiment, as well as many others, prove undoubtedly that there is an increase of sensibility in these last parts. I will try in another article to explain this hyperæsthesia.

I believe I am entitled to conclude from the facts above related:

1st. That most of the impressions made on one side of the body are transmitted to the sensorium by the opposite side of

the spinal cord, so that the impressions on the left side of the body are transmitted by the right side of the spinal cord, and *vice versa*.

2d. That the assumed function of the crossing of fibres in the pons Varolii, and the neighboring parts, does not belong to these fibres, but to the fibres of the spinal cord, all along which they cross each other.

XXI.—ON MUSCULAR IRRITABILITY IN PARALYZED LIMBS, AND ITS SEMEIOLOGICAL VALUE.

Marshall Hall has published many papers, in which he has tried to prove that the degree of muscular irritability in paralyzed parts may be used as a means of diagnosis between *cerebral* and *spinal* paralysis.

He calls *cerebral* paralysis that in which the paralyzed part is deprived of the action of the brain, but not entirely, or not in the least, of the influence of the spinal cord. On the contrary, he calls *spinal* paralysis that in which the palsied part is altogether deprived of the action of both the brain and the spinal marrow. The cause of the cerebral paralysis may be seated either in the encephalon or the spinal cord; and the cause of the spinal paralysis may be seated either in the spinal cord or in the nerves.

In the same individual these two kinds of paralysis may exist together. Suppose a man in whom the brachial enlargement of the spinal cord is considerably softened, and consequently unable to act; the upper limbs then have a spinal paralysis, and the lower limbs, receiving their nerves from a healthy part of the spinal cord, have only a cerebral paralysis.

According to Marshall Hall, the cerebral paralysis is attended by augmented muscular irritability, and the spinal paralysis is attended by diminished irritability. He bases this opinion on the following experiments, and on some clinical observations.

On six frogs he divided the spinal marrow immediately below the origin of the brachial nerves; and he removed a portion of the ischiatic nerve of the right posterior extremity. He had immediately, or more remotely, the following interesting phenomena:

1st. The anterior extremities alone were moved spontaneously; both posterior extremities remaining entirely motionless when the animal, placed on its back, made ineffectual efforts to turn on the abdomen.

2d. Although perfectly paralytic in regard to spontaneous motion, the left posterior extremity, that still in connexion with the spinal marrow, moved very energetically when stimulated by pinching the toes with the forceps.

3d. The right posterior extremity, or that of which the ischiatic nerve was divided, was entirely paralytic, both in reference to spontaneous and excited motions.

4th. After the lapse of several weeks, whilst the muscular irritability of the left posterior extremity was gradually augmented, that of the right was gradually diminished,—phenomena observed when the animal was placed in water through which a slight galvanic shock was passed accurately in the direction of the mesial plane.

5th. Strychnine being now administered, the anterior extremities and the left posterior extremity, or that still in connexion with the spinal marrow, became affected with tetanus; but the right posterior extremity, or that severed from all nervous connexion with the spinal marrow, remained perfectly placid.

6th. Lastly, the difference in the degree of irritability in the muscular fibre of the two limbs was observed, when these were entirely separated from the rest of the animal.

After this exposition of the results of his experiments, Marshall Hall adds: "In a word, the muscles of the limb paralyzed by its separation from both cerebrum and spinal marrow, had lost their irritability; whilst those of the limb separated from its connexion with the cerebrum only, but left in connexion with the spinal marrow, not only retained their irritability, but probably possessed it in an augmented degree.*

It is easy to prove that Marshall Hall has been completely misled by his experiments.

It is well known that the more a muscle is excited, the more it contracts. As the degree of irritability is judged by the degree of the contraction, it follows that to know what is the degree of muscular irritability we ought to apply the same excitation to the muscles we desire to compare. In his experiments with galvanism and with strychnine, Marshall Hall has not done so. He has applied

* On the diseases and derangements of the nervous system. 1841, p. 215.

galvanism, so as to excite much more the muscles of the left side united with the spinal cord, than those of the right side.

The muscles of the left side were excited :

1st. Directly by the galvanic current.

2d. In consequence of the excitation of the motor nerves.

3d. In consequence of the excitation of the spinal cord, directly by the galvanic current, and secondarily in consequence of the excitation of the sensitive nerves.

So that the muscles on that side were moved not only by the direct excitation on them, but also by a reflex action, and in consequence of the direct excitation of the spinal cord.

As to the muscles of the right side, they were only excited by the small part of the galvanic current passing in them. During the first, and perhaps the second and the third week after the section of the ischiatic nerve, the muscles were also slightly excited by the motor fibres of that nerve, but after that time these fibres had lost their vital property, and were unable to excite a contraction in muscles.

From this analysis it results clearly that the mode of comparison of the two limbs, by the passage of a galvanic current, as it has been employed by Marshall Hall, could not decide in which side the muscles were more irritable.

The use of strychnine, also, could decide nothing in this question, because, as I have proved in a former article, this poison is not able to act upon muscles. It acts only on the nervous centres, and especially on the spinal cord. Therefore, the production of tetanus in one limb and not in the other, in the experiment of Marshall Hall, proves nothing at all as to the degree of muscular irritability.

To know what is that degree, it is necessary to separate the two limbs from the trunk, and then to excite directly the muscles. Marshall Hall has made this experiment, but he says nothing about the circumstances under which it was performed, and these circumstances were, as it will be shown, extremely important.

In my experiments, instead of dividing only the ischiatic nerve, I divided the four nerves going to one of the posterior limbs, of many frogs, in whom the spinal cord was divided immediately behind the roots of the brachial nerves.

I have found on the separated limbs of these frogs :

1st. That, at first, the muscular irritability was greater in the limb which had been deprived of the action of the spinal cord and of the brain, than in the limb deprived only of the action of the brain.

2d. That, at a variable time after the operation, the irritability was at the same degree in the two limbs.

3d. That, at last, the irritability became greater in the limb only deprived of the action of the brain, than in the other.*

The differences in the degree of irritability have been observed: 1st. By the degree of the contraction under the influence of the same excitation; 2d. By the duration of irritability.

I have found that during a time, varying much according to seasons, and to many other circumstances, the muscular irritability increases in the two posterior limbs in a frog operated upon as I have described, and that the increase was more considerable in the limb where the nerves were divided than in the other.

If we compare two frogs, one operated on as before, and another having only had a division of all the nerves on one of the posterior limbs, we find, a few days after the operation, that in the four limbs separated from the body there are great differences as to the degree and the duration of muscular irritability: 1st. The three paralyzed limbs have a greater irritability than the one not at all paralyzed. From the three paralyzed limbs the two in which the nerves have been divided have both the same degree of irritability, and more than the limb in which there was only what Marshall Hall calls a cerebral paralysis. 2d. The irritability has lasted longer in the two limbs in which the nerves had been cut, than in the two other limbs; and from these two, that in which there was a cerebral paralysis has remained longer irritable.

If we examine the irritability in the posterior limbs of two frogs, operated on as aforesaid, for ten, twelve, or fifteen days, then we find that it is nearly at the same degree in the three paralyzed limbs, and greater there than in the non-paralyzed limb.

* There is, in these experiments, a cause of error, arising from the existence on one side, and the absence, or, at least, a diminution in the other, of the vital power of the motor nerves; but the difference is trifling when the nerves are divided very near their entrance in the muscles.

If the comparison is made four or five weeks after the operation, then the non-paralyzed limb has a greater irritability than the three paralyzed, and, from these three, the one deprived only of the cerebral action has a greater irritability than the two others.

The same experiments made on other animals than frogs, i. e. on guinea-pigs and rabbits, have given like results. I shall publish the details of these last experiments in a special paper, in which I intend to examine the value of the clinical observations of Marshall Hall, R. B. Todd, Duchenne de Boulogne, and others. I will merely state here, that in many cases it is almost impossible to know what is the difference in the degree of muscular irritability in a paralyzed limb, compared with a healthy limb, in a living man or animal. Galvanism and strychnine cannot give us any exact notion in this respect. I ought to add, that if we could know what is the relative degree of irritability in a paralyzed limb, we could not make use of that knowledge for the diagnosis of the seat of the alteration producing the paralysis. On the other side, we do not want to know what is the degree of irritability in order to establish such a diagnosis. It will be, almost constantly, easy to know whether a paralysis is a cerebral or a spinal one. The existence of reflex actions in the paralyzed parts, is sufficient to prove that there is a cerebral paralysis, and the absence or the slight degree of these actions will prove that there is a spinal paralysis.

The following conclusions may be drawn from the facts above related, and from others that I have not yet published.

1st. The degree of muscular irritability in paralyzed parts, becomes rapidly greater than in the healthy parts, but, after a variable length of time, it diminishes, and, as it is well known, it may disappear.

2d. The muscles deprived of the action of both the brain and the spinal marrow, become rapidly more irritable than the muscles deprived only of the action of the brain, but, after a certain time, there is also in them a more rapid diminution of irritability than in the others.

3d. It appears certain that the muscular irritability never

disappears completely in parts deprived only of the cerebral action.*

4th. In certain cases of paralysis, and more particularly of the face, as after the removal of a large part of the facial nerve, the muscular irritability may exist for years, at least in rabbits and other animals.

5th. It is very difficult, and sometimes almost impossible, to know the relative degree of muscular irritability in healthy parts compared with paralyzed parts, and such a knowledge could not be of a great semeiological value.

6th. The existence or the absence of reflex actions as a means of diagnosis between the cerebral and the spinal paralysis, has a much greater value than the degree of muscular irritability.

XXII.—ON THE INCREASE OF ANIMAL HEAT AFTER INJURIES
OF THE NERVOUS SYSTEM.

In another part of this series† I have endeavored to prove that the local increase of temperature following the section of the sympathetic nerve, is the result of paralysis of the blood-vessels. I will now relate some other cases in which a local increase of temperature takes place after various other injuries of the nervous system, and apparently in consequence of the same cause.

It was known, long ago, that an injury to the nervous system might be followed by a partial or even a general elevation of animal heat. Sir B. Brodie says,‡ Mr. Chossat has published an account of some experiments on animals, in which he found that the division of the superior portion of the spinal cord produced a remarkable evolution of animal heat, so that it was raised much above the natural standard. I have made experi-

* I have had a pigeon on which nearly an inch of the costal part of the spinal cord had been removed, and on which the muscular irritability in the posterior limbs, and a very great reflex power, have existed as long as I have taken care of it, *i. e.* more than twenty-seven months. I ought to say that there has been no re-union of the separated parts of the spinal cord.

† Medical Examiner, August 1853, p. 489.

‡ Medico-Chirurg. Transactions, 1837. Vol. xx., p. 132.

ments similar to those of Mr. Chossat, and have met with similar results. I have also seen several cases in which an accidental injury of the spinal cord has produced the same effect. The most remarkable of them was that of a man who was admitted into St. George's Hospital, in whom there was a forcible separation of the fifth and sixth cervical vertebrae, attended with an effusion of blood within the theca vertebralis, and laceration of the lower part of the cervical portion of the spinal cord. Respiration was performed by the diaphragm only, and, of course, in a very imperfect manner. The patient died at the end of twenty-two hours; and, for some time previous to his death, he breathed at very long intervals, the pulse being weak and the countenance livid. At last there were not more than five or six inspirations in a minute. Nevertheless, when the ball of a thermometer was placed between the scrotum and the thigh, the mercury rose to 111° of Fahrenheit's scale. Immediately after death, the temperature was examined in the same manner, and found to be still the same.

Brodie was mistaken as regards the experiments of Chossat. Instead of finding an increase in the animal heat after the section of the inferior portion of the spinal cord, Chossat found a considerable diminution in the temperature of dogs. But in two cases, where the spinal cord was divided at about the level of the last dorsal vertebra, in dogs, Chossat* found an increase in the animal heat. In one of these experiments, the increase was from $41^{\circ}.1$ to $41^{\circ}.5$ Cents., ($105^{\circ}.98$ to $106^{\circ}.7$ Fahr.) In the other, the increase was from $41^{\circ}.1$ to $42^{\circ}.9$ Cs., ($105^{\circ}.98$ to $109^{\circ}.6$.)

Dr. Macartney† found an increase in the temperature of parts paralyzed in consequence of the division of their nerve. H. Nasse,‡ who made many experiments on this subject, sometimes observed an elevation in the temperature of the paralyzed parts after the division of the sciatic nerve, or after the partial destruction of the spinal cord.

* Mém. sur l'influence du syst. nerv. sur la chal. anim. Thèse de Paris. No. 126.—1820, p. 35. Exps. xxiii and xxiv.

† Treatise on Inflammation, 1838, p. 13.

‡ Vutersuchunger zur Physiol. und Pathol., 1839, v. ii., p. 190.

In more than twenty experiments, I only once found an increase in the temperature of the leg of a guinea-pig, after the section of the sciatic nerve. This increase lasted about two or three days after the operation, and it was of two degrees Fahr.

After a complete transversal section of the spinal cord in the lumbar region, in birds and mammals, I found, repeatedly, an increase of one, two or three degrees Fahr. in the temperature of the paralyzed parts. I ascertained that it is not in consequence of an increase of the general temperature of the animal that such an increase exists. It is to be found only in the paralyzed parts.

I never found any increase of temperature after a complete transversal section of the spinal cord, either in the cervical or in the dorsal region.

After a section of a lateral half of the spinal cord, at the level of one of the three or four last dorsal vertebræ, I have almost constantly found an increase in the temperature of the posterior limb on the side of the section. The elevation varied from one to four degrees Fahr. On the contrary, there was a diminution of from one to five degrees Fahr. in the temperature of the other leg. In some cases, in consequence of the increase of temperature on one side and its diminution on the other side, I found a difference of six or seven degrees Fahr. in the temperature of the two limbs. It is very remarkable that, together with the increase of temperature in one limb, there is an augmentation of sensibility, and with the diminution of temperature in the other limb, there is also a diminution of sensibility.

Since the publication of the results of my experiments on the sympathetic nerve, I have performed them many other times, and I have found that the result is not so constant as Dr. A. Barnard and myself had admitted. In some rabbits there was no decided increase in the vascularization and in the temperature of the face. I ought to say that, in these cases, the two ears were already warm, and very vascular before the operation. I have found, also, that generally in very cold weather the extremity of the ear of rabbits, on the side of the section of the sympathetic nerve, remains cold.

From my experiments and from the observations and experi-

ments of Brodie, Chossat, H. Nasse and Macartney, it results that the following opinion of Dr. Cl. Bernard is incorrect. He says: "It is known that injuries of the cerebro-spinal nervous system constantly produce a total or a partial diminution in the temperature of animals, either when a nerve has been divided or when the injury is made on the nervous centres."* He says also that an injury of the sympathetic nerve produces a very rapid increase of temperature; so that the sympathetic nerve and the cerebro-spinal nervous system are considered by Dr. Bernard as completely different, one from the other, as to the influence on animal heat when they are injured. The one should increase and the other diminish animal heat.

The truth is that these two effects—increase and diminution—may exist after an injury of either the sympathetic or the cerebro-spinal nervous system;† and, in both cases, the increase may exist, at first, and be followed by a diminution.

Before pointing out the co-existence of certain facts with the increase or diminution of animal heat, I think it necessary to establish a distinction between the cases of increase of animal heat after injuries of the nervous system.

In some cases (as those related by Brodie) there has been an increase of temperature above the natural standard of animal heat. These are very extraordinary and very rare cases, and it is not my intention to attempt to explain them here. In the cases, the degree of temperature, although increased in some other paralyzed parts, has not been above the normal degree of blood heat. This is the only kind of increase of animal heat that I have observed, and this I will attempt to explain.

I have found that,—*ceteris paribus*,—the more the arteries and capillaries are dilated, the higher is the degree of temperature. This law is proved by the following facts:

1st. In all the cases of paralysis (from whatever cause) where I have found a diminution in the degree of temperature of a paralyzed part, the arteries and capillaries were evidently much contracted.

2d. In the cases where the temperature was normal, the blood-vessels were of their natural size.

*Gaz. Medic. de Paris, Vol. 7, No. 14, p. 227.

† Vide : Chossat, *loco cit.*, pp. 41-46.

3d. In the cases where the temperature was increased, I have constantly found the arteries and capillaries enlarged.

4th. In some cases, I have found the same changes occurring in the temperature and in the blood-vessels. The temperature at first was greater than usual and the blood-vessels dilated; afterwards both the temperature and blood-vessels became natural; and, at last, the temperature becoming lower than usual, the size of the blood-vessels became smaller.

I need not say that the changes occurring in paralyzed parts in accordance with the size of the blood-vessels were the results of the differences in the amount of blood passing in these parts.

Now, it will be asked how, in certain cases of palsy, the size of the blood-vessels is larger than usual, and smaller in other cases. I cannot explain how it is so, but I can assert that it is a fact.

From the facts and reasonings related in this article, I draw the following conclusions :

1st. An injury of the nervous system may produce in the parts, which then become paralyzed, either an increase or diminution of temperature.

2d. The sympathetic nerve and the cerebro-spinal nervous system appear not to be different one from the other, in this respect.

3d. The degree of temperature of paralyzed parts depends on the quantity of blood they receive; and this quantity varies according to the size of the arteries and capillaries of these parts.

4th. It is a fact, hitherto unexplained, that the arteries and capillaries may be either dilated, normal, or contracted in paralyzed parts.

XXIII.—CAUSE OF THE STOPPING OF THE HEART'S MOVEMENTS, PRODUCED BY AN EXCITATION OF THE MEDULLA OBLONGATA OR THE PAR VAGUM.

E. II. and E. Weber have discovered a singular fact, hitherto unexplained. When the par vagum or the medulla oblongata is excited by a powerful electro-magnetic current, in a living animal, the movements of the heart are suddenly stopped. This

should be what is known for all motor nerves and muscles, if the cessation of the movements of the heart was the result of a permanent contraction. But the heart is not at all contracted, and, on the contrary, it remains perfectly placid.

This is entirely different from what we know to be the case for other muscles.

I have found that a violent mechanical excitation of the medulla oblongata produces also the same stopping of the heart's action.

Is the heart in a state of rest in consequence of a loss of its irritability or of an interruption of the excitation necessary to its action? The following fact proves that this second opinion is the right one. When the heart is stopped, every direct excitation upon it produces some beatings, and then, its irritability appears to be entire. The stopping, consequently, depends on the absence of excitation.

The cause exciting the heart to beat is in the blood contained in the capillaries of this organ, as I will try to prove in another article. Now, if we suppose that the galvanization of the par vagum produces a complete constriction of the capillaries of the heart, it is easy to understand why the heart is stopped: it is because the excitation cannot take place on account of the expulsion of the blood from the capillaries.

It will be asked on what ground we base the supposition that the capillaries are so contracted that they prevent entirely, or nearly so, the passage of the blood. I will answer:

1st. That it is known that a galvanization of certain nerves (and I have discovered that it is so with the capillaries of the face and ear when the sympathetic nerve is galvanized) may produce a considerable constriction of capillaries.

2d. That it is known that the nerves of the heart are distributed much more to its blood-vessels than to its muscular tissue.

3d. That, by our supposition, we place the fact of the stopping of the heart's movements among the well known facts, that an excitation of a motor nerve produces a contraction of the muscles to which it is distributed; and, therefore, we are not obliged to admit that an excitation of a nerve is able to produce directly either a contraction of or the cessation of existing contractions.

There is a practical consequence to be drawn from the fact that

in the case of an excitation of the medulla oblongata, the stopping of the heart is not produced by a loss of irritability of this organ.

Many cases of syncope are produced by a stopping of the heart's movements in consequence of the influence of an emotion on the medulla oblongata. In these cases it would be of very great importance to excite directly the beatings of the heart, either by compression of the chest or by an application of galvanism.

We ought to say that galvanism applied directly to the heart increases its beatings instead of diminishing them.

XXIV.—ON A SINGULAR DISTURBANCE IN THE VOLUNTARY MOVEMENTS, APPARENTLY PRODUCED BY AN ACTION OF ATMOSPHERIC AIR ON THE GRAY MATTER OF THE SPINAL CORD, IN BIRDS.

Some years ago I discovered that after the removal of a large quantity of the gray matter that exists in birds, on the posterior surface of the spinal cord, in the lumbar region, a great disturbance took place in the voluntary movements. I attributed this disturbance to the loss of gray matter. I have found, three or four months since, that the same disturbance existed in the voluntary movements after I had merely laid bare the gray matter, and immediately after it had been exposed to the action of air.

I am perfectly satisfied that it is not in consequence of a mechanical excitation of the spinal cord, accidentally produced during the operation of the removal of the bones and membranes, that this disorder takes place.

When the spinal cord is laid bare elsewhere than in the region of the lumbar enlargement—that is, in any place where the white substance covers completely the gray matter—there is no disturbance produced in the voluntary movements.

That disturbance very much resembles the so-called titubation which exists after either the removal of the cerebellum or the section of muscles of the posterior part of the neck. At each movement of progression, the animal tends to fall either forwards, backwards, or laterally. It does not fall completely, but is obliged, in order to avoid falling, to make use of its wings, its tail and its beak.

XXV.—ON THE TREATMENT OF EPILEPSY.

I have made numerous experiments with regard to the treatment of this dreadful affection, and I intend to publish them, in extenso, when some points that are still obscure have become clear to my mind. Here I will merely relate some of the most important results of my researches. As I have had the opportunity during the last three or four years of observing every day a great many animals (more than a hundred) which had a convulsive affection resembling epilepsy very much, I have been able to discover some very interesting facts, among which are the following :

1st. For each epileptic animal, the number of fits, in a given time, is generally in a direct proportion with the quantity of food taken.

2d. There is an inverse proportion between the amount of exercise and the number of fits.

3d. Cauterisation of the mucous membrane of the larynx is able either to cure or to relieve these epileptic animals.

The convulsive affection existing in almost all these animals was the consequence of a transversal section of a lateral half of the spinal cord, in the dorsal or in the lumbar region.

I have already published the results of my experiments on epilepsy, in my lectures before large classes of Physicians and Medical students, both in France in 1851 and in this country in 1852.

These results are in perfect accordance with the views of Dr. Marshall Hall in relation to epilepsy. As the views of this eminent biologist are generally known, I need not expose them, and I will merely remind my readers of the three following points :

1st. The first muscles that contract spasmodically in almost all, if not in all, the cases of epileptic fits, are those of the larynx and the neck : 2d, spasm of the glottis taking place then, produces suffocation, in consequence of which convulsions are produced in the trunk and the limbs ; 3d, tracheotomy may prevent these convulsions by preventing suffocation, and it is known that in some cases tracheotomy has cured epilepsy.

It has been objected to Marshall Hall that in cases of poisoning by strychnine, convulsions take place even when a tracheal tube renders respiration perfectly free. This objection has no

value, because the state of the spinal cord in epileptics is not the same as in men or animals poisoned by strychnine. Certainly the excitability of the spinal cord is greater in epileptics than in healthy persons, but the degree of excitability of that nervous centre is much greater in persons poisoned by strychnine than in epileptics; and, therefore, it is easy to understand that certain excitations are able to produce general convulsions in one case and not in the other.

If we give a very slight dose of strychnine to an animal, so as not to poison it, but merely to increase slightly the excitability of the spinal cord, there are no convulsions when we touch or pinch or burn the skin, but if we prevent breathing for a few seconds only, general convulsions take place, exactly as in epileptic men or animals.

It has been said also, in opposition to Marshall Hall, that a spasm of the glottis of the severest kind occurs in cases of hooping cough, of spasmodic croup and even of apoplexy, without the occurrence of any other convulsions. The answer to this objection is, that in epilepsy the spinal cord is more excitable than in these other diseases, so that the same kind of excitation does not produce the same effects.

A great many facts, that I will publish elsewhere, prove that black blood, very probably by its carbonic acid, is an excitant of the spinal cord and of the medulla oblongata. When, as is the case in asphyxia, the blood is not oxygenated and deprived of the carbonic acid constantly produced in it, or received by it from different tissues, then the excitation made on these nervous centres becomes so powerful that convulsions are produced. This is found in men and animals, even in perfect health. If the asphyxia is incomplete, convulsions are not produced, unless the excitability of the spinal cord is greater than usual, and this is the case in epileptics.

In November, 1851, at the *Ecole Pratique*, of Paris, I published for the first time, before a class of about forty young Physicians and Medical students, the results of my experiments as regards the cauterization of the larynx in epilepsy. About eight months after, Dr. Eben Watson published a paper* in

* Remarks on Dr. M. Hall's theory of the relation of Laryngismus to Epilepsy. In London Journal of Medicine, July, 1852, pp. 611-13.

which he says : "The treatment I would now propose instead of tracheotomy is simply the application of a solution of nitrate of silver, varying in strength with the requirements of the case, to the glottis of the patient, with the view of diminishing the nervous excitability of the part in question. A similar treatment has been found by me remarkably successful in alleviating and removing, in a short time, the susceptibility of the patient to laryngismus, in cases of hooping cough, and of spasmodic croup (*laryngismus stridulus*), nor can I see any reason why a similar result should not ensue in chronic cases of epilepsy."

The reasons given by Dr. E. Watson are partly the same by which I had been led long before him to perform the operation he suggests. But I had also some other reasons. It is perfectly known, in the actual state of Medical Science, that the greatest changes may be produced in the nervous centres, as well as in the nerves, by a very strong excitation of the termination of the nervous fibres in the skin or the mucous membranes. On this principle are founded many modes of treatment of some diseases of the spinal cord and of neuralgia. The application of caustics, blisters, cupping, hot iron, etc., is based on this principle. In accordance with it I am inclined to believe that epilepsy might be cured by a mere application of a hot iron to the skin of the neck ; at least I have had two guinea-pigs cured after such an application, repeated three or four times.

The operation of tracheotomy proposed by Marshall Hall has proved successful in some cases. But it is a dangerous operation, and if it is proved that another one much slighter can produce the same good effects, it ought not to be practised.

That other operation is the cauterization of the larynx ; it prevents the closure of the glottis, and thus is able to cure or to relieve epileptic patients as well as it cures some other diseases. Every learned physician knows that it is sufficient to cauterize the larynx once or twice to cure hooping cough in almost every case.

When the cause of the epileptic fits is excessive, and when the spinal cord is very excitable, to allow free breathing merely will not be sufficient to prevent the general convulsions. But their violence, if respiration is free, will be deprived of all the effect that would be produced by the excitation of black blood if breathing did not take place.

The distinction made between organic and inorganic epilepsy has not the importance that some writers seem to admit. There are alterations in the nervous system in both cases, and the only difference is that these alterations can be easily seen with the naked eye in one case and not in the other. I ought to point out that the cases of epilepsy in animals, which I have cured, were cases of organic epilepsy. These animals have been cured, although the apparent and primitive cause of the disease, *i. e.* a section of a lateral half of the spinal cord, continued to exist.

The cauterisation of the larynx on these animals was made every day, or every other day, and sometimes during two or three months. In some cases, the relief having been immediate, the cauterisation was made only twice a week. One of the animals experimented on was cured after three or four cauterisations; but the number of cauterisations necessary has been generally very much greater. When I left France in February, 1852, I had cured about a third of the animals treated by this method; and all the others, except two or three, had been very much relieved, and certainly many of them would have been cured if the treatment had been prolonged.

I knew that an animal was cured, not only by the absence of spontaneous fits, but when I could not produce a fit by giving great pain. I had found that on any epileptic animal, except immediately after a paroxysm, I could very easily produce a fit by exciting pain and more particularly by pinching or burning the skin of the face or neck. So that I am authorised to believe that when a fit was not produced by pinching or burning the face, it was because epilepsy had ceased to exist.

Some physicians in this country have already tried on man the mode of treatment that I have found so successful on animals. From what I know of the results of their attempt, it seems to me that man is like animals in this respect. There has not been yet a complete curation: but, except in one case, there has been a very considerable diminution in the frequency and the intensity of the fits.

As physicians who have to treat epileptics, have not to make experiments, but to cure by making use of all the best means together, I think that the treatment of epilepsy ought

not to consist merely of the cauterisation of the larynx. The plan of treatment I should suggest is the following :

1st. A cauterisation of the larynx with a strong solution of nitrate of silver, (at least 60 grains to the ounce,) every day, for at least five or six weeks.

2d. A cauterisation of the skin of the neck over the spine, with a hot iron, once a fortnight, for about two or three months.

3d. Exercise and gymnastics.

4th. Make use of oxide of zinc or ammoniated copper, remedies which a very respectable physician of Geneva, (Switzerland,) Dr. Herpin, has found successful in many cases, when their dose has been considerable.*

5th. If in a fit of epilepsy the suffocation is very considerable, the operation of tracheotomy ought then to be performed immediately.

XXVI.—CURE OF EPILEPSY BY SECTION OF A NERVE.

It is a well known fact that epilepsy may be produced by injury to a nerve. Dr. John Cooke, in his *Treatise on Nervous Diseases*, says on this subject : “ From the writings of Forestus, Van Swieten and Tissot, it appears that injury done to the nerves, or that a morbid state of them, has in many instances given occasion to epilepsy. In the *Edinburgh Medical Essays and Observ.*, a case is related of a violent epilepsy, which frequently occurred, which was produced by a hard cartilaginous substance, of the size of a large pea, situated upon a nerve. That this was the cause was evident, as the disease ceased on the extirpation of the tumor. In the same we have an account of epilepsy depending upon a calculus of an irregular figure, about the size of a nut, pressing on a branch of the sciatic nerve ; and another in which the par vagum was compressed by a concretion of a similar kind.” Darwin, in his *Zoonomia*, says, “ I once saw a child about ten years old, who frequently fell down in convulsions, as she was running about in play. On examining, a wart was found on one ankle, which was ragged and inflamed, which was cut off, and the fits never recurred.”

* See his admirable work: *Du Pronostic et du Traitement de l'Epilepsie*. Paris, 1852. Ouvrage couronné par l'Institut de France.

Van Swieten relates a case of curation of epilepsy by the extirpation of a hard cartilaginous body, somewhat larger than a pea, situated on a nerve of the leg.

A case is related in the *Medical and Physical Journal*, (vol. x. p. 52,) in which a cure of epilepsy was effected by the application of caustic to the nerve which accompanies the vena saphena.

Many other analogous cases are on record. Jacques Carron, (*Recueil périodique de la Soc. de Médecine de Paris*, t. xviii. p. 422,) cured a child by the extirpation of a small sebaceous tumor which existed on one of the fingers. Portal, (*Observations sur l'épilepsie*, Paris, 1827, p. 159,) cites a case observed by Fabos, in which the fits were preceded by a pain in one of the fingers. During a violent fit Fabos put a ligature around the radial nerve, and the patient was completely cured. Portal (*Anatomie Médicale*, vol. iv. p. 247,) relates that one of his pupils, Mr. Leduc, cured an epileptic by the extirpation of a hard tumor which was on one of the fingers. Joseph Frank cured by castration a patient in whom epilepsy had appeared after an injury to the scrotum. Henricus ab Heer, cited by Sennert, (*Opera omnia*, vol. ii. p. 489,) having observed that during her fits, a girl used to rub her two big toes, cured her by the application of caustic to these toes. Similar cases have been related by Alexander, of Tralles, and by Wepfer.

I have cured a guinea-pig of a very violent convulsive affection, much resembling epilepsy, by a section of the sciatic nerve. This animal had been bitten by another on the toes of one of the posterior limbs. A considerable slough appeared on the wounded part, and after two or three weeks fits appeared, and the animal shortly afterwards had many very violent fits every day. I laid bare the sciatic nerve and cut it transversely. After this operation I kept the animal many months, and never saw it have a fit. From this fact, and from those observed by many physicians above related, it appears clearly that in cases of epilepsy it is necessary to examine if there is no injury whatever to some nerve, and more particularly when the *aura epileptica* exists. If there is such an injury, the treatment ought to be either the section of the injured nerve or the removal of the tumor, if there is one, and sometimes the application of a caustic or a blister.

XXVII.—LAWS OF THE DYNAMICAL ACTIONS IN MAN AND ANIMALS.*

The following laws are based upon a very considerable number of facts which I have observed or found on record in many books, pamphlets and journals. I have collected these facts and I intend to publish them in a special paper.

I ought to say that many Physiologists, and more particularly Fontana, Delaroche, Adamucci, Broussais, Buchez, Réveillé-Parise, J. Mueller, J. Paget and Carpenter, have pointed out the existence of some parts of some of these laws.

1. Nervous actions, muscular contraction, contraction of the cellular tissue, the discharge of the electrical apparatus of some fishes, the galvanic current of certain organs, the galvanic discharge which accompanies the muscular contraction, and *probably* also the phosphorescence of certain animals and the ciliary movements, are phenomena which cannot exist without being attended with an organic waste which nutrition alone can provide for.

2. The faculty of originating these phenomena has a tendency to increase in direct ratio to the rapidity of the circulation of blood, to its abundance, and to the amount of its nutritive materials, both general and special.

3. During rest, *i. e.* at the time of the non-existence of these phenomena, the tendency of such a faculty towards augmentation meeting with no obstacle, augmentation actually takes place.

4. The increase is much more rapidly effected when an action has just been performed, than it is after a prolonged rest.

5. Nutrition becoming altered in the tissues which remain inactive for a long time, the faculty of producing the above-mentioned phenomena diminishes by degrees, and even finally disappears when the structure of the tissues has been deeply modified.

6. The faculty of originating these phenomena increases in direct ratio with the length of the rest, within certain limits; and when the latter are overleaped, there is a period when no change takes place; but afterwards the faculty decreases, on the contrary, in direct ratio with the length of the rest.

* I ought to say that these laws and the facts upon which they are established, have been the subject of many communications that I have made to the *Société de Biologie*, at Paris, in the year 1848.

7. For many tissues which produce these phenomena a complete rest is scarcely possible. The phenomena take place, in appearance, spontaneously, and with as much energy as the temperature is higher.

8. The faculty of producing these phenomena decreases at the time they are going on, in proportion to their intensity and duration, and in inverse ratio to the nutritive reparation which simultaneously takes place.

9. Reparation thus incessantly supplying for expenditure, it is not possible, with regard to most of these phenomena, and as long as circulation goes on, to destroy entirely the faculty of originating them; or rather, as soon as we have succeeded in destroying that faculty, it is reproduced by nutrition.

10. Expenditure dependent upon action, being followed by a great activity in the nutrition, it happens that, if the action be frequently renewed, there is an excess of nutrition and a considerable increase of the faculty of acting.

11. When the faculty of acting has been increased in virtue of the preceding reasons, within a certain limit, an equilibrium exists between the expenditure and the reparation, and the increase no longer takes place.

12. As it is possible for nutrition to take place in the tissues, although the nutritive fluid is not actually circulating in them, and provided that a certain amount of it exists in them, the faculty of acting may be increased in parts where circulation is stopped.

13. Although circulation and consequently reparation, are more active in summer than in winter time, at least in cold-blooded animals, the faculty of acting becomes more considerable in winter than in summer time, because the spontaneous expenditures abovementioned, and those due to external stimuli, or dependent upon the will, are by far less considerable.

All the preceding laws may be summed up in the following:

The intensity of the faculty which animal tissues possess, of producing the vital phenomena, seems to be in a direct ratio to the intensity and duration of the nutritive reparation, and in an inverse ratio to the intensity and duration of the existence of these phenomena.

XXVIII.—INFLUENCE OF RED BLOOD ON MUSCLES AND NERVES
DEPRIVED OF THEIR VITAL PROPERTIES.

James Phillips Kay* has found that blood, injected into limbs of dead animals, just after irritability has disappeared, is capable of regenerating this vital property. I have gone much farther, and have discovered that blood is able to regenerate the vital properties of nerves and muscles, even in limbs which have lost their irritability and have been rigid for several hours. I have obtained this result from the following experiments:

1st. On the body of a rabbit, in which cadaveric rigidity had already existed for 10, 20, and in one case, 33 minutes, I divided the aorta and the vena cava in the abdomen, immediately above the bifurcation of these vessels. By means of small tubes, a communication was established between their peripheric extremity and the central extremity of the corresponding vessels divided in a living rabbit. The blood of this living animal circulated immediately in the posterior limbs of the dead one. After about six, eight or ten minutes, rigidity disappeared, and, a few minutes afterwards, movements took place when I excited the muscles or the muscular nerves.

2d. I have obtained a like result from an experiment more easily made than the preceding, and which I have performed more frequently. I divided transversely the body of a living guinea-pig, or rabbit, into two halves, on a level with the lower border of the kidneys, leaving no communication between the two halves, except by the aorta and the vena cava. I then tied the aorta immediately below the origin of the renal arteries. The muscular irritability gradually diminished, and in a very variable length of time† it gave way to cadaveric rigidity. I waited until rigidity had been fully developed in all the muscles, and then the ligature was relaxed and the circulation re-established. Rigidity disappeared slowly, and the muscles and the motor nerves resumed their vital properties.

3d. In order to ascertain if voluntary movements and sensibility could be restored to limbs that had been in a state of ca-

*Treatise on Asphyxia. London, 1834

† Sometimes 30, 20, or even only 10 minutes in weak animals, and from 1 to 8 or 9 hours in strong animals.

daveric rigidity, I tied the aorta immediately behind the origin of the renal arteries, in several rabbits. Shortly afterwards, sensibility and the voluntary movements disappeared in the posterior limbs. I waited until muscular irritability had given way to what is called *cadaveric* rigidity; and when that peculiar rigidity had existed for at least twenty minutes, I relaxed the ligature. Then circulation took place, and, in consequence of it, sensibility and voluntary movements re-appeared.

From this experiment it results, that not only local life, but all the properties and actions of full life, can be restored in limbs that have been in the state called *rigor mortis*, *cadaveric* or *post-mortem rigidity*.

4th. On a man, 20 years old, who was guillotined on the 18th of June, 1851, in Paris, I made an experiment similar to some of the preceding. The decapitation took place at 8 o'clock A. M. Ten hours afterwards, *i. e.* ten minutes after 6 o'clock P. M., the muscles of the hand, upon which I intended to experiment, exhibited some slight manifestations of irritability. At 7 and at 7½ o'clock P. M. I ascertained that they had lost their irritability. Shortly after they were in a state of *cadaveric* rigidity.

I began the injection of blood 10 minutes after 9 o'clock P. M.

As I wished to inject fresh human blood, and as I could not obtain any from the hospitals at such an hour, I was obliged to make use of my own. My friends, Drs. F. Bonnefin and Deslauriers, drew from one of the veins of my left arm half a pound of blood, which was immediately beaten and completely defibrinated and filtered through a cloth.

As, in opposition to the general opinion, I had found that it is not necessary, in transfusion, to make use of blood at a temperature not far from that of warm-blood animals, I left the blood employed in this experiment freely exposed to the atmosphere during all the time of the operation. The temperature of the air was 19° centigr. (66°.2 Fahr.) I regret not having taken the temperature of the blood when I began to inject it, but it was probably about the same as that of the atmosphere.

The injection was made into the radial artery, a little above the wrist. The whole quantity of the blood was injected in about 8 or 10 minutes. The arm operated on had been separated from

the body, and the blood injected came out from all the divided arteries and veins.

Having saved nearly all the blood which flowed from these vessels, I injected it anew. The last injection was made 45 minutes after 9 o'clock P. M. Ten minutes afterwards I found that cadaveric rigidity had ceased in the hand, and that two muscles only, out of the nineteen existing in that part, had not resumed their irritability. Three muscles had become so very irritable that a slight mechanical excitation was followed by a contraction in the whole length of their fibres.

At half past one o'clock A. M.,—seventeen hours and a half after decapitation and four hours after the injection of blood,—there was still a slight irritability in the muscles of the hand.

In this experiment I found that half a pound of defibrinated human blood was sufficient to give irritability, for several hours, to seventeen of the muscles of a hand.*

5th. An experiment on another guillotined man gave me more interesting results. The decapitation had taken place at 8 o'clock A. M. on the 12th of July, 1851. At 5½ o'clock P. M., cadaveric rigidity existed in almost all the muscles of the arms and fore-arms. I separated them from the body, and at 6½ o'clock I ascertained that cadaveric rigidity was increased, and that only a few muscles were still slightly irritable. At 8 o'clock P. M. (12 hours after the decapitation) the muscles of the two arms were completely deprived of irritability, and in full rigidity, and the muscles of the forearms contracted only locally under the influence of a mechanical irritation, and not at all when excited by a powerful magneto-electric current. Two other examinations made, one at 9½ and the other at 10 o'clock, gave the same results.

At 10¼ o'clock two or three bundles of fibres of one of the muscles of the fore-arm were the only parts where a mechanical excitation produced a slight local contraction. All the other muscles were perfectly stiff and deprived of irritability.

Twenty-five minutes after 10 o'clock there was no appearance of irritability remaining in any muscle.

I then began the preparations for the injection of blood, with

* For a full account of the circumstances of this experiment, see my paper in the *Gaz. Medic. de Paris*, t. vi.—1851, p. 421.

the assistance of Drs. Martin-Magron, F. Bonnefin, Crouzet, and Mr. Moyses.

We drew about a pound of blood from the carotid of a strong dog. The blood was beaten and defibrinated before coagulation could take place in it, and 10 minutes after 11 o'clock the injection was begun. It was made in the brachial artery of the left arm, in the middle of its length, where the arm had been amputated. As soon as the blood had been thrown in the artery, some reddish spots appeared in different parts of the skin of the fore-arm, of the hand, and more particularly of the wrist. These spots became larger and larger, and the skin had the appearance it has in rubeola. Soon after, the whole surface of the skin was of a violet reddish hue. In a few minutes this color disappeared, and was replaced by the natural hue of the skin during life. The skin became elastic and soft, as in a living man, and we saw the bulbs of its hair becoming erected and presenting the appearance called *cutis anserina*. By increasing and diminishing alternately the impulsion given to the blood, we succeeded in producing the beatings of the pulse in the radial artery. The veins were distinct and full as during life.

A short time after, the fingers, which had been extremely stiff, relaxed, and rigidity disappeared also in the other parts of the limb.

Forty-five minutes after 11 o'clock P. M., irritability had returned in all the muscles of the limb operated on. The degree of irritability, more particularly in the muscles of the arm, (triceps, biceps and others) was very considerable, and much greater than I had seen it at the time the corpse was first examined (about five o'clock P. M.) Irritability was still present in almost all the muscles of this limb at 4 o'clock A. M., (20 hours after the decapitation,) when I was obliged, from extreme fatigue, to abandon further investigation.

The blood injected was at 23° centig. (73°.4 Fahr.) when I began the operation, and the atmosphere was at 19¼° centig. (66.66 Fahr.)

In this experiment, about one pound of defibrinated dog's blood gave irritability for more than five hours to all the muscles of a limb, from the middle of the arm to the hand.

6th. Every one knows the singular fact, that Vibriones and

other Infusoria, when desiccated, will live when they are put into water. It is also perfectly known that seeds, after many centuries, may grow when put in the earth. I have found something of the same kind in higher organisms; it is that muscles, in a certain condition, after having been separated from the body for many days, may recover their irritability.

Dr. Coze, of Strasbourg, has found that chloroform injected into the main artery of a limb produces instantly the strongest rigidity, and that if blood is allowed to circulate again in the limb, life appears again in it. I have gone farther, and found that if a limb, in which an injection of chloroform has been made, is separated from the body, it is able, under the influence of an injection of blood, to recover its muscular irritability 2, 3, 4, 5 and (in one case) 10 days after the rigidity was produced. I think Mr. Edouard Robin is right in admitting that chloroform prevents the chemical changes that take place in organic bodies after death, and, if it is so, we can understand why an injection of blood made so long after the limb has been separated from the body, may reproduce irritability. One day is not more than one hour, if, during it, there is no alteration produced in the muscles.

It appears, nevertheless, that chloroform does not entirely prevent the alterations of muscles, because, in my experiments, I have found that the longer the limbs had been separated from the body, the greater was the quantity of blood necessary to reproduce irritability.

7th. I lately made an experiment, with the view of ascertaining how long a limb, separated from the body of an animal, may be kept alive by means of injected blood. I succeeded in retaining local life in one of the limbs of a rabbit more than 41 hours. The animal was a very vigorous, full grown one. I killed it by hemorrhage, and, two hours afterwards, rigidity had begun in most of the muscles of the two posterior limbs, and only a few bundles of muscular fibres had still a slight irritability. A first injection of defibrinated blood was then pushed in the femoral artery of the right posterior limb. Fifteen minutes after the beginning of the injection, local life (*i. e.* irritability) was restored in the limb receiving blood, and cadaveric rigidity had disappeared.

The manner of testing this irritability was the same as that of

Glisson, Gorter, and all the experimenters of the two last centuries,—I mean by mechanical excitation. I did not use galvanism, as it exhausts muscular irritability too much, as Autenrieth, Pfaff and many other observers have shown long ago. Being aware of this fact, I have always, in my preceding experiments, made use of galvanism for a very short time only.

Three hours after the death of the rabbit, irritability still existed in the right limb (the injected one,) while the left was perfectly rigid and had not the slightest irritability. Half an hour later, rigidity had begun again in the right limb; blood was injected anew, rigidity disappeared, and local life returned. From this moment until 11 o'clock, P. M., (death had occurred at 6 o'clock, A. M. of the same day,) blood was injected many times. Rigidity did not return, and the vital property of the muscles was maintained. Of course the left limb, during that time, remained rigid, and had not the slightest irritability.

From 11 o'clock, P. M. until 6 o'clock, A. M. the succeeding day, an abundant injection of blood was made every twenty or twenty-five minutes. The irritability was not powerful, but it existed in all the muscles of the limb. There was no rigidity at all.

The injections were then made more frequently—once in each quarter of an hour—until three o'clock, P. M., at which time I was obliged to stop them for an hour and a half.

At half past four I found the limb rigid, and only a few bundles of muscular fibres still irritable. A very abundant injection was then practised, and rigidity soon disappeared, giving way to irritability. From this time to 11 P. M., a great many injections were made, and irritability was maintained. I was then obliged to give up the experiment. At that moment irritability was strong in all the muscles of the injected limb, except some parts of their pelvic extremities that had not received a sufficient quantity of blood.

The next morning that limb was in full and energetic rigidity. The other limb had already lost its rigidity, and had an evident smell of putrefaction. The third day after the death of the animal, rigidity was strong in the injected limb, while the other was in an advanced state of putrefaction.

If we compare these two limbs, we find, 1, That the injected

one had a strong irritability at the end of forty-one hours after the death of the animal; 2, That its rigidity gave way to putrefaction only at the eightieth hour; 3, That it was in complete putrefaction only at the ninety-fourth hour. The other limb was in full rigidity at the fifth hour after the death of the animal; its rigidity gave way to putrefaction at the forty-eighth hour; and it was in complete putrefaction at the seventieth hour.

From all the experiments above related, it appears that life may be reproduced or maintained in muscles and nerves by mere injections of blood. I have found, also, that life may be reproduced by the same means in the spinal cord and in the brain. I will publish these facts in another article.

It is nearly indifferent in these experiments whether we use venous or arterial blood; but it is absolutely necessary to employ red blood, *i. e.* oxygenated blood.

I have tried, sometimes, arterial blood, rendered black by the substitution of nitrogen or hydrogen for a great part of its oxygen, and I have found that such blood was unable to reproduce the vital properties of nerves and muscles.

Oxygen is necessary, either because it prevents the blood-globules from being altered, or because it acts directly on muscles, as Gustavus Liebig has found it does on their external surface, when exposed to air. I believe it is necessary for both these reasons.

I cannot say how long after the beginning of cadaveric rigidity in a muscle, oxygenated blood can reproduce local life. In the second of the two decapitated men, on whom I experimented, rigidity had existed at least five hours before the injection was begun. I believe that the stronger the animal is, the more easy it is to reproduce local life in rigid limbs, by injection of blood. In limbs of weak rabbits, I have found it impossible, two hours after the beginning of cadaveric rigidity, to reproduce local life. In a very strong dog I have reproduced muscular irritability four hours after rigidity had been fully developed.

Ten, twelve, or fourteen hours after rigidity had taken place, in human limbs, I have tried in vain to re-establish local life.

I have ascertained that pure serum of blood, or milk, or albu-

men of eggs, are unable to produce any apparent change in rigid limbs.

The following conclusions are to be drawn from the facts related in this article:

1st. Red blood, *i. e.* richly oxygenated blood (arterial or venous) is able to revive irritability in muscles, four or five hours after these organs have lost this property.

2d. Red blood is able to revive the vital properties of nerves and nervous centres, when these properties have not been lost for more than about an hour.

3d. Muscular irritability can be maintained for more than 41 hours, by mere injections of blood, in limbs separated from the body of a rabbit.

4th. Muscular irritability may be re-established in limbs rendered rigid by chloroform for many days, even ten days.

XXIX.—CASES OF LOSS OF SENSIBILITY ON ONE SIDE OF THE BODY.
AND LOSS OF VOLUNTARY MOVEMENTS ON THE OTHER SIDE.

It has been objected to me that if the transmission of sensitive impressions, in the spinal cord, takes place, as I have tried to prove in a former part of this sketch (Art. XIX,) so that those coming from the left side of the body, are mostly conveyed to the sensorium along the right side of the spinal cord,—*et vice versa*—physicians should have some times found in man the same thing that I have discovered in animals.

Many reasons have prevented physicians from making such a discovery: In the first place, an injury or a pathological alteration, limited to a lateral half of the spinal cord, is very rare. Besides, the idea that there is no crossing of fibres in the spinal cord, has been an obstacle to a thorough examination of many pathological cases, and it has been so in a case observed by Boyer.

There are but few cases on record in which there was a loss or a diminution of sensibility on one side, and of voluntary movements on the other. I will give here a short account of some cases of that kind, which are very interesting.

The first one I will relate has been observed by Boyer:

A drummer, of the National Guard of Paris, received a wound in the back of his neck. A sword had been thrown at him, and had penetrated the superior part of the right lateral half of the

neck. An incomplete paralysis of movement took place in the right side of the body, and, some time after, it was accidentally discovered that sensibility was lost in many parts of the left side of the body. After twenty days the wound was cured, and the man went out of the hospital, still paralysed.

From what we know of that case, it appears that the sword had incompletely divided the right lateral half of the spinal cord. The paralysis of motion on the right side of the body was certainly produced by the division of a part of the anterior column, and, as the instrument had penetrated the right side of the back of the neck, it must have divided the parts between the anterior column of the spinal marrow and the external surface of the right side of the neck. These parts, besides the muscles and bones, are the lateral and posterior columns and the gray matter of the right half of the spinal cord. So that in this case nearly the same injury and also the same morbid phenomena had existed as in the animals on which I have divided a lateral half of the spinal cord.

The following case is still more interesting. It has been recorded by Dr. R. Dundas, Surgeon of the Hospital of Bahia.

A mason fell on his back from an height of 20 feet. After having recovered his consciousness, he discovered that all the left side of his body, from the shoulder to the foot, was paralyzed as to motion, without the slightest alteration of sensibility, and that the right side in which the movements were free, was completely deprived of sensibility.

Three important facts, precisely like those I have discovered in animals after the transversal section of a lateral half of the spinal cord, existed in this case :

1st. A morbid exaltation of sensibility in the side where movement was lost.

2d. A diminution of temperature in the side where the paralysis of sensibility existed.

3d. An increase in temperature in the side where the paralysis of movement existed.*

* In a former part of this sketch (Art. xxii.) I have related facts proving that animal heat may be increased after injuries to the spinal cord. I have learned since, that Prof. D. Gilbert has observed a case of fracture of the spine, in which the temperature of the paralyzed parts was increased. Prof. Dunglison has also stated that the paralyzed side in hemiplegic patients may have an elevation of temperature.

When Dr. Dundas published this curious case, the patient was living and improving; so we do not know what was the alteration existing in the spinal cord.

H. Ley, in a letter to Sir Charles Bell, relates the following case:†

Mrs. W., after a profuse hemorrhage, became paralytic. Upon one side of the body there was a loss of sensibility, without, however, any corresponding diminution of power in the muscles of volition. The breast, too, upon that side, partook of the insensibility, although the secretion of milk was as copious as in the other. She could see the child sucking and swallowing, but she had no consciousness, from feeling, that the child was so occupied.

Upon the opposite side of the body there was defective power of motion, without, however, any diminution of sensibility. The arm was incapable of supporting the child; the hand was powerless in its grasp; and the leg was moved with difficulty, and with the ordinary rotatory movement of a paralytic patient; but the power of sensation was so far from being impaired that she constantly complained of an uncomfortable sense of heat, a painful tingling, and more than the usual degree of uneasiness from pressure, or other modes of slight mechanical violence.

She again proved pregnant. Her delivery was easy: but after about ten days she complained of numbness on both sides. Her articulation was indistinct; she became more and more insensible, and sank, completely comatose.

No positive disorganization of the brain could be detected. The ventricles, however, contained more than usual serum; and there were found thickening and increased vascularity of the membranes, with moderately firm adhesion in some parts; in others, an apparently gelatinous, transparent and colorless deposit interposed between them.

Unfortunately, no examination of the spinal cord was made.

In this case there was very likely, as in my experiments, an increase in the temperature of the side paralyzed of motion. The writer merely says that *the patient was constantly complaining of an uncomfortable sense of heat*. There was, as in my animals, an evident increase in sensibility on that side.

†The nervous system of the human body. By Ch. Bell. 3d ed. London, 1844, p. 245.

M. Monod* has related the case of a man who, after having felt a sudden pain in his back, became paralyzed in the motion of the right inferior limb. Sensibility was entire on this side, but on the left side, where the movements were entire, sensibility was entirely lost from the breast to the foot. There was at first no fever. The patient died 34 days after the beginning of this affection.

The brain and its membranes were normal. A hemorrhage had taken place, and blood was found in the right side of the central gray matter, in the neighborhood of the anterior column in the dorsal and lumbar regions.

This case is assuredly a very remarkable one, and in accordance with my experiments.

The conclusion to be drawn from these four cases is, that in man as well as in animals, there appears to be a crossing of the sensitive nerve-fibres in the spinal cord.

XXX.—ON THE DIFFERENT DEGREES OF EXCITABILITY OF THE DIFFERENT PARTS OF THE SENSITIVE NERVE-FIBRES.

It is a well-known fact, that an excitation of the skin or of a mucous membrane, produces a greater pain or a greater reflex action than that of the nerve trunk, from which these parts receive their nerve-fibres. For instance, a slight excitation of the laryngeal mucous membrane produces coughing, while an excitation of the vagus nerve very rarely produces the same effect. Therefore, there is a notable difference between the peripheric extremity of a nerve-tube and its part contained in a nerve-trunk.

The existence of a peculiar organ in the skin (the *corpuscles of touch* of Wagner) has not much (if it has anything) to do with the different degrees of excitability of nerve-tubes in the skin and in the trunks of nerves. The corpuscles of touch do not exist in the mucous membranes, and if they exist in the skin of frogs, turtles, etc., it is in a very small number; and, nevertheless, the degree of excitability of nerve-fibres in these parts is much superior to that of the fibres of the nerve-trunks.

Some very striking differences exist in the degree of excita-

* Bulletin de la Société Anatomique, No. xviii. p. 349.

bility of centripetal nerve-fibres in the five following different parts of their length.

1st. The part contained in the skin.

2d. The part of a nerve extending from the skin to the spinal cord.

3d. The posterior roots of the spinal nerves.

4th. The part of the posterior roots attached to the spinal cord.

5th. The part of the cutaneous nerve-fibres contained in the gray matter of the spinal cord.

The fibres existing in the gray matter of the spinal cord appear to be inexcitable, at least by our ordinary means of excitation. Of the four other parts, the less excitable is the nerve between the ganglion and the skin. The excitability of the posterior roots is less than that of the skin and that of their part attached to the spinal cord. Of these two last parts the skin is less excitable than the other.

I measured the excitability by the degree of pain or of reflex action. The differences are much more easily found for the reflex action than for the pain.

Is it because they have been connected with the cells of the central gray matter of the spinal cord, that the centripetal fibres, contained in that gray matter, are not excitable? If it is so, there is a difference between these cells and those of the ganglions on the posterior roots, because the connection of these fibres with the cells of these ganglions does not prevent their being excitable.*

From the facts above related I conclude that the same nerve-fibre, in different parts of its length, may have very different degrees of excitability.

XXXI.—THE AUDITIVE NERVE IS A NERVOUS CENTRE.

In an anatomical point of view there is no doubt that the auditive nerve is a nervous centre. This is proved by the fact that cells of gray matter are found, not only in the terminal part of the nerve, but also in its trunk, in many animals, according to the researches of Stannius, Corti, Kölliker, and myself.

In a physiological point of view, the fact I have discovered, (see Art. V. p. 21,) viz., that any injury to the acoustic nerve

produces turning, is sufficient to prove that it is a nervous centre.

The degree of pain produced by an excitation of this nervous centre appears to be as considerable as that caused by a similar excitation of the trigeminal nerve. I will publish soon an account of the strange effects produced in different parts of the body in consequence of an injury of that nervous centre. I will merely say here that, after such an injury, there are muscles which appear to be slightly paralyzed. Besides, there seems to be a notable hyperæsthesia of the skin everywhere.

Flourens has found that a section of the semi-circular canals in birds and some mammals produces a peculiar disorder in the movements of the head, and, in some cases, turning. He says that the auditive nerve must be considered as composed of two nerves: one going to the semi-circular canals and possessing a peculiar power on the movements of the body, and the other, the vestibular or true auditory nerve. What I have found on frogs is in opposition to these views. A section of the semi-circular canals, in these amphibia, does not produce any effect on the movements of the body, and the slightest excitation of the true auditive nerve is sufficient to produce pain, hyperæsthesia, turning, and other strange effects on many muscles of the body.

I have sometimes seen turning produced after the mere laying bare of the kind of bladder, containing the terminal part of the auditive nerve, in frogs. So slight may be the excitations on that nerve sufficient to produce turning, that very likely turning after the laying bare of that bladder was the result of some slight mechanical injury of the nerve. The rapidity of turning and the smallness of the circle then described are in proportion to the degree of injury to the nerve. When the two auditive nerves are injured, the animal turns on the side most injured. Sometimes, instead of turning, the animals roll around the longitudinal axis of their body; this takes place in very strong animals after the terminal part of the nerve has been entirely crushed.

In frogs deprived of their cerebral lobes, the same effects are produced after injuries of the auditive nerve, as in unmutilated frogs.

XXXII.—ON APPARENTLY SPONTANEOUS ACTIONS OF THE CONTRACTILE TISSUES OF THE ANIMAL BODY.

All the contractile tissues of the animal body (the muscles of the trunk and limbs, the muscular layers of the digestive canal, the iris, the uterus, the dartos, the cellular tissue, etc.) present, sometimes, apparently spontaneous contractions. I give this name to contractions which are not the result of an external excitation or of an excitation produced by the nervous system on the contractile tissues. These contractions may be permanent or momentary, rhythmical or irregular, slight or very powerful. One of their causes, if not their only cause, appears to be an excitation directly produced on the contractile fibres by the carbonic acid existing in the blood.

1. *Contractions in the muscles of the face after a section of the facial nerve.*—My friend Dr. Martin-Magron and myself have discovered that after the section of one of the facial nerves, on a rabbit, the face becomes very quickly deviated, not on the healthy side, as it is known to be in man, but, strange to say, on the paralysed side. The deviation, very slight at first, increases gradually during one or two weeks, and then it is so considerable that the middle of the lips is at a distance of four, five or six lines from its natural situation. There is an evident state of contraction in all the paralysed muscles. When the animal is excited, or when its respiration is somewhat disturbed or prevented, the paralytic muscles tremble, and sometimes they have rhythmical contractions and relaxations.

The contractions of these muscles may be so considerable that the bones themselves, and, secondarily, the teeth, may be deformed. In one case, on a rabbit which I had kept living twenty-one months after the extirpation of one of the facial nerves, not only the superior and inferior jaws were by far less developed on the paralysed side than on the other, but the anterior part of the superior maxillary bone was deviated towards the paralysed side, so that the middle line of the roof of the mouth was curved and presented a great concavity on the paralysed side and a corresponding convexity on the other.

When the two facial nerves have been divided, there is no

deviation, but there is an evident state of contraction in all the paralysed muscles, particularly around the lips.*

When one of the facial nerves is divided on a dog, on a cat, or on a guinea pig, there is generally no deviation on either side. But very frequently there are convulsive movements, and sometimes rhythmical contractions, in the paralyzed side of the face. One of these two kinds of movements always exists in young cats. They are increased, or produced when they do not exist, in dogs and guinea-pigs, almost every time we prevent the animal from breathing freely. Once, on a very vigorous guinea pig, upon which one of the facial nerves had been torn away, I saw alternate contractions and relaxations taking place, without a relapse, for eight or ten days after the operation in the paralysed muscles. After that time, these tremblings appeared only when the circulation and the respiration were rendered very active, or when the respiration was prevented or diminished. In the case of an impaired respiration, the strength and frequency of these movements were in proportion to the degree of asphyxia. During many months, the same phenomena existed in this animal.

I ought to say that in all the experiments above related, the nerve could not have any share in the movements, because, the fifth day after the division, or after the extirpation of a portion of it, the peripheric part had entirely lost its vital property.

In man, as Dugès justly remarks, as long as there is no attempt at movement, voluntary or emotional, the face remains without any deviation, in cases of facial hemiplegia, which have not lasted a long time.

2. *On spontaneous rhythmical or irregular contractions in muscles of animal life, after death.*—It is a very important fact in connection with the theory of the action of the heart, as I will try to prove hereafter, that other muscles, and particularly muscles of animal life, are capable of having rhythmical movements. This fact I have discovered in the following cases:

a. After the division of the nerves of the ischiatic and lumbar

* Dr. Martin-Magron and myself have found that death occurs from inanition in all the species of mammals on which we have divided the two facial nerves. After the operation they cannot swallow: we do not know why.

plexuses, on one side, in mammals, if we suddenly asphyxiate the animal, we see, at first, convulsive movements in the three limbs and in other parts of the body not paralysed. After one, two or three minutes, these movements cease, and there are only some tremblings in the muscles of these parts. The paralysed limb has no movement at all during one or two minutes, after which time, suddenly, contractions in many bundles of muscular fibres partially take place. In the same bundle the contractions sometimes appear to come regularly one after the other. In some cases I have seen, besides these tremblings, movements of the entire limb, consisting of some successive flexions and extensions of the limb, and after these movements had ceased, contractions limited to various bundles of fibres appeared. In these cases the action of the muscles began very late after death, and once, only six minutes after the beginning of asphyxia, which lasted two minutes and a half.

b. Nearly the same movements of which I have spoken as existing frequently in the face, during life, in rabbits and guinea-pigs, after the section of the facial nerves, exist always, either during agony or a little after death. They are generally produced by partial contractions and relaxations of the different bundles of fibres of the various muscles. It is rare to see all the bundles composing one muscle contracting together. These phenomena last five, six or eight minutes after the last respiration. There are also such movements in the face during agony and after death, when the nerves have not been cut and when there is no paralysis; but then the movements appear later and do not last so long as in paralysed muscles.

c. I have seen in many rabbits apparently spontaneous rhythmical contractions in the respiratory muscles. In about ten rabbits, out of forty or fifty, the following phenomena were very decided; on the others they were slight, and sometimes very slight, but in all cases a part of them always existed. I open the abdominal cavity and expose the bowels to the action of a cold atmosphere, so as to lower the temperature of the animal; after some minutes I make a little opening in one side of the chest, and, at last, after a few minutes more, I open largely one side of the chest. Generally, in such circumstances, the respiratory movements continue to take place with energy.

I then take away the sternum and divide the two diaphragmatic nerves. The movement of the diaphragm, nevertheless, continues, and it exists rhythmically together with the movements of the other respiratory muscles. Six, eight or ten minutes afterwards the movements of the diaphragm are still regular, (there are from five to twenty contractions in a minute;) the intercostal muscles present then only partial contractions. The different bundles of fibres of these muscles contract separately one after the other, but the same bundle has generally regular contractions and relaxations. At that time I destroy the spinal cord, and see that the movements of the diaphragm and of the intercostal muscles are not changed after this operation; they last for nearly a quarter of an hour, and in some cases much longer; their regularity subsists. In the diaphragm, long after the general movement has stopped, there are regular or irregular contractions of many bundles of fibres for one, two, three hours, and sometimes more.

3. *Deviation of limbs produced by a contraction of paralysed muscles.*—In pigeons, after the destruction of all the lumbar part of the spinal cord, the two posterior limbs are completely paralysed. The muscles then are soft, and the different parts of the limbs do not resist at all, when we try to put them in flexion or in extension. But after a few days the paralysed muscles become harder, and after a few weeks there is an evident state of contraction in them. The limb is generally kept in a state of extension, and deviated on one side or the other. The deviation becomes considerable after some months.

Very likely it is owing to the same cause that club-foot and other deviations are produced in embryos, after a destruction or an absence of development of the spinal cord.

4. *Rhythmical movements in the eye of the Ink-fish.* (Loligo sepia, L.)—The ciliary muscle so well described by Dr. W. Clay Wallace, of New York, in the eyes of superior animals, is strongly developed in the ink-fish. After an eye of this mollusc has been separated from the body, I have sometimes found very singular and perfectly rhythmical movements produced by the ciliary muscle. These movements consisted in alternative contractions and relaxations of some parts of that muscle. At every contraction a notable depression was produced in one portion of a zone

corresponding to the circumference of the cornea.* In one case I have found four times in fifteen minutes the same rhythm existing in one part of the ciliary muscle. At each of these four examinations I have found sixteen contractions in one minute.

5. *Spontaneous Contractions of the Uterus.*—I have seen hundreds of times the uterus or its cornua, full or empty, contracting to appearance spontaneously, after the death of rabbits and other animals, at a time when the spinal cord had entirely lost, not only its reflex power, but also the power of acting on muscles when directly excited by galvanism, by warmth or mechanically.†

I have also seen movements taking place in the uterus and in its cornua, in recently dead animals, the spinal cord of which I had destroyed in all its length. The same movements I have found after I had taken out from the abdomen of a living animal the whole uterine apparatus. I have found sometimes that after I had put a ligature around the trachea of guinea pigs, which were at the end of gestation, parturition took place and was produced by three causes: 1st, a direct excitation of the spinal cord by the venous blood; 2d, a direct excitation of the uterus by that blood; 3d, a reflex action of the spinal cord. In two cases I have seen delivery taking place after the action of one only of these three cases, namely, the direct influence of black blood on the uterus of the Guinea pigs, the spinal cord of which I had destroyed from the sixth costal vertebra to the sacrum. The more complete and sudden is the asphyxia, in a rabbit or a Guinea pig, during labor, the more certain will the delivery take place.

Dr. Tyler Smith speaks of a peristaltic action of the uterus, which may expel the child when the mother has died during labor, undelivered. He has not attempted at all to explain that con-

* The eyes had not been opened.

†Dr. Tyler Smith, in his very original book on Parturition, (London, 1849, p. 40,) says that "a slow reflex action of the uterus may possibly continue long after the rhythmic respiratory actions have ceased; as long, indeed, as the body retains its warmth." There is a great error in these lines, about the relation between the warmth of the body and reflex action. We may observe reflex actions even in animals that have lost 10, 12 or 15° Cents., (18, 22 or 27° Fahr.,) of their temperature, and, in certain circumstances, these actions may be, then, more powerful than if the temperature of the body was normal. For instance, if we decapitate an animal after having

traction, *i. e.* to find out its cause and the circumstances which favor or are opposed to its existence; besides, he has not demonstrated that the peristaltic contraction is entirely independent of the nervous system.

6. *Spontaneous rhythmical movements in the crop and œsophagus of pigeons and other birds.*—I have found that if the crop of a bird, and more particularly of a pigeon, is opened during digestion, some rhythmical movements are frequently seen in it and in the œsophagus. Ordinarily these movements are perfectly regular. They begin in the upper part of the crop, and are propagated from there to the œsophagus. If the animal is asphyxiated, these contractions become very energetic. Their ordinary number, in a minute, varies from ten to twenty.

I have ascertained that these rhythmical movements take place as well in a crop and œsophagus separated from the animal, as in these same parts left *in situ*. Therefore, the nervous centres are not the source from which originates the excitation which acts on the muscular fibres to put them in contraction.

7. *Spontaneous movements in limbs of persons who have died of cholera.*—It is known that after death by cholera, the whole body, and more particularly the limbs, have sometimes very considerable movements. In some cases I have seen alternative movements of flexion and extension of the arms or of the legs, even three hours after the cessation of the beatings of the heart. Physicians who know how quickly after death the nervous system loses its vital powers, will admit easily that these movements cannot be the result of an action of that system. I have ascertained on more than sixty bodies of men who died of cholera, or of various other diseases, that a short time before, or a very short time after the cessation of the beatings of the heart, no reflex

put a ligature around the carotid and vertebral arteries, we find, when pulmonary insufflation is made carefully, that two important phenomena take place—one is a gradual rapid loss of temperature, if the atmosphere is cold, (this is the well known fact discovered by Sir B. Brodie,) and the other is a gradual and considerable increase of the reflex faculty. It has been in such cases that I have found the greatest degrees of reflex power in mammals. The nervous power accumulates to such an extent in the spinal cord, that if we pinch the skin in any part of the body, but more particularly on the chest and on the anterior limbs, a reflex respiratory movement takes place.

action was produced by the tickling of the sole of the foot. The greatest duration of reflex action that I have observed after death has been in a case of cerebral apoplexy. It has lasted thirteen minutes after the last breathing, and about eight minutes after the last beating of the heart. Dr. Bennet Dowler has recorded many curious facts (observed in cases of death from yellow-fever, cholera, etc.,) from which he concludes also that the movements taking place in the limbs are not reflex actions.

I have found that, in general, the more sudden and complete has been the asphyxia before death, by cholera, the more the limbs are moved after death. I have found also that it is in patients who have died during the algid period that these movements are ordinarily found.

These facts, as I will show hereafter, appear to prove that these movements, like the other movements, of which I have previously spoken, are excited by carbonic acid alone, or together with the poison of cholera.

8. *Spontaneous contractions of the bowels, the bladder, the iris and other parts of the body.*—It is known that frequently at the time of death, many of the contractile tissues of the body are put into contraction. I can go farther and say that it is so with *all* the contractile tissues; and that, contrary to the general opinion, a nervous action is not necessary for these contractions.

There are contractions in all the following organs or tissues during agony and after death: 1, the muscles of animal life; 2, the sphincter of the anus; 3, the respiratory muscles; 4, the iris; 5, the digestive canal (in all its length); 6, the urinary bladder; 7, the uterus; 8, the scrotum, (dartos); 9, the gall-bladder; 10, the ureters; 11, the seminal vesicles; 12, the bronchial tubes; 13, the skin; 14, the blood-vessels; 15, the lymphatics; 16, the cilia.

As to the skin, in many cases the so-called *goose-flesh* (cutis anserina) takes place a little before or little after death, although the body has not yet become cold. I have seen it very strongly marked on the inferior limbs of a paraplegic who died of a softening of the dorso-lumbar part of the spinal cord. It results from this fact that the cellular tissue is able to contract from the same

cause which produces contractions at the time of death, in muscular tissues—that is, very likely, carbonic acid.*

Besides, I have found contractions of the cellular tissue of the skin of the face, in animals killed by asphyxia, and on which the facial nerve had been divided for many days or weeks.

In the same man who had a paraplegia, and of whom I have just spoken, I saw very strong contractions in the dartos, during agony.

In animals suddenly asphyxiated, after the destruction of the dorso-lumbar part of the spinal cord, the seminal vesicles sometimes contract, and a slow ejaculation takes place, although there is no erection.

In the sphincter of the anus, when it is paralysed, there are only slight contractions, but they are evident.

The urinary bladder, during agony or after death, sometimes contracts so much, even when it is paralysed, that all the urine it contains is expelled.

The ureters present very strong contractions, in animals recently killed by asphyxia, and these contractions in some cases are rhythmical. The same movements are seen when all the urinary apparatus is *in situ*, and when it has been removed from the abdomen, and therefore separated from the nervous centres. The contraction begins at the kidney and thence is very quickly propagated all along the ureters to their termination in the bladder. Among the contractile tissues, that of the ureters is one of the most irritable.

Bidder and Schmidt, of Dorpat, have recently found that after the division of the two pneumogastric nerves, there is more carbonic acid expelled by the lungs than usual. This fact is very

* Kolliker has recently discovered fibro-muscular cells—that is, muscular fibres of organic life—in the skin, and he maintains that the *cutis anserina* is produced by these fibres, and not by the cellular fibres. I have published facts which, I think, prove conclusively that the contractions in the skin are in a great measure performed by the cellular tissue. (See *Comptes Rendus de la Soc. de Biologie*, 1849, t. i. pp. 134 et 157, et 1850, t. ii. p. 132.) Since that time, I have found that in some cartilaginous fishes, in which the iris does not contain any muscular fibre, and is composed of cellular tissue, this membrane may be the seat of considerable contractions; so that I consider it as perfectly certain that the cellular tissue (at least in some organs) is contractile.

important, because if the theory, which I am about to propose, be true, we ought to see a contraction produced in the bronchial tubes, in consequence of the unusual amount of carbonic acid that they contain. Now, such a contraction certainly exists then, and it is it which causes the well-known difficulty in the expansion of the chest, which exists in that case.

In the eyes, even when they are paralysed by the section of the three nerves of the iris, (the third pair, the sympathetic, in the neck, and the ophthalmic nerve,) the pupil may, at first, contract and afterwards dilate very much.

The lymphatics and the thoracic duct contract very much after death. I have, sometimes, in cases where these vessels were dilated by chyle, introduced a glass tube, two lines in diameter, into the thoracic duct, and I have seen the liquid ascend into the tube, and in one case run out, although the tube was five inches high.

The cilia are known to have movements independent of the nervous system.

The gall-bladder contracts little and slowly, but evidently, after death, even when it has been, with the liver, removed from the abdomen and separated from the nervous centres.

The choledoch duct and the pancreatic duct, as my friend Cl. Bernard has discovered, have rhythmical contractions during life, in birds. I have found these movements perfectly regular after I had removed all the viscera from the abdomen. Therefore the cause of these rhythmical contractions is not in the nervous centres.

The bowels have considerable contractions during agony and after death; and I will prove hereafter that the cause of these movements is not the influence of cold, or that of air, when they are exposed to the atmosphere. Nurses, in France, are in the habit of judging that death has positively taken place, when, after the cessation of breathing, they see urine and faecal matters expelled. This expulsion depends upon the contractions then taking place in the bladder and in the bowels.

9. *Causes of the apparently spontaneous contractions during life and after death.*—All the contractions of which I have spoken, appear to me to be produced by an excitation made upon the contractile tissues by a substance existing in the blood, and

the quantity of which becomes much increased during asphyxia. The relations between these contractions and asphyxia are evident. A great many of them do not exist unless asphyxia exists, and their energy is always in proportion to the degree of asphyxia.

I believe that the substance in the blood which has that power is the carbonic acid. In admitting this opinion we can easily explain all the phenomena.

There are certain contractions which take place in muscles of animal life, after death, and which have quite another cause. In the cold seasons, it is not uncommon to find, in limbs of frogs, when we separate them from the body, apparently spontaneous contractions, lasting sometimes for half an hour or even more; but these contractions have begun when we have cut the nerves, and they continue on account of galvanic discharges which accompany them. The fact that they begin after the excitation of a nerve, is sufficient to show that they are not like the other contractions, of which I have previously spoken.

Some of the facts I have related may appear to be distinct from the others. So, for instance, contraction taking place in paralysed muscles of the face or of the limbs in living animals, might be considered as quite different from the contractions existing after death. I think that they originate from the same cause, viz., an excitation by carbonic acid. A muscle may be moved or not be moved by an excitant. If the degree of irritability is greater in one case than in another, we may see the same amount of excitation produce a movement in the first case, and not in the second. If the amount of excitation increases, then we may see both muscles moved, but the most irritable more than the other. This is sufficient to explain why the paralysed muscles may be moved by the carbonic acid existing in the blood during life, while the muscles that are not paralysed are not moved. I have found that the degree of irritability increases, during a certain time after paralysis, in the muscles of animal life. Their irritability being augmented, they are excited sufficiently to contract, by a quantity of carbonic acid which is not sufficient to act on the other muscles.

The following facts and reasonings will, I believe, prove that, at least in the bowels, black blood, very likely by its carbonic

acid, may excite powerful movements. It is known that when we open the abdomen of an animal immediately, or a short time, after death, we generally see considerable movements in the bowels. These movements have been attributed to the action of air, or to that of cold, on the bowels. This is not a right view. A sudden exposure to a cold atmosphere may, possibly, produce contractions in the bowels; but certainly cold is not the ordinary cause of these movements. At first, they may exist in a warm atmosphere, and then they appear to be more rapid than in a cold atmosphere. Besides, the bowels may be exposed to a cold atmosphere, and remain motionless, although they have their entire irritability. As to atmospheric air, it is not able to excite a movement in the bowels. If we open the abdomen of a living animal, in avoiding to excite mechanically the bowels, and in allowing the animal to breathe freely, we may for a long time see no other movement in the bowels, except, sometimes, slight regular and natural peristaltic motions, depending on digestion, and limited to some small parts of the bowels. The animal must be kept on his back, and we must avoid touching the bowels, because a slight contact is sufficient to produce movement. Now, if we prevent the animal from breathing, we see, after ten, fifteen, or twenty seconds, very violent, sudden, and rapid contractions taking place in all parts of the intestine, from the stomach to the rectum, but much more in the small intestine than elsewhere. These movements are quite different from the digestive peristaltic movements. If the animal is allowed to breathe again, and freely, the movements diminish gradually, and disappear almost entirely after a few minutes. Then, if we prevent it again to breathe, we see the movements produced again. This experiment may be repeated many times, with the same result, on the same animal.

We are certainly entitled to conclude that there is an exciting cause of contractions, developed during asphyxia, and that it is neither the cold nor the atmospheric air which produces in all cases the movements of the bowels after the opening of the abdomen. We may draw the same conclusions from another experiment. If we put a tie around the trachea of a living animal, immediately after expiration, we may see and feel violent movements taking place in the bowels, although the abdomen is not

opened. It is in consequence of such movements that there is an expulsion of faecal matters, after death, in man. The urine may be also expelled in these cases, in man and in animals, and this expulsion takes place because the bladder contracts, and not, as it is generally admitted, because the *sphincter vesicæ* becomes relaxed.

Some physiologists have considered the cessation of the circulation of the blood in the bowels as the cause of their movements, after death, and they relate as a proof the fact that the section of one of the arteries going to a part of the intestines, is followed by contractions in the parts thus deprived of circulation. But nothing is explained by saying that the cause of the contraction is in the absence of circulation. As contractions require an excitation to be produced, what is the exciting cause when the blood does not circulate? After the section of an artery there is blood remaining in the capillaries, and that blood, after a short time, becomes very rich in carbonic acid, and then, if my theory is right, contractions ought to be produced. The result of the section of one of the arteries is, therefore, in accordance with my theory.

Other facts may be adduced proving the influence of black blood and carbonic acid on the bowels.

If black blood is injected in the arteries of the small intestine when its irritability is much diminished, movements are almost immediately produced, but they do not last long. On the contrary, if red blood is injected, movements do not appear immediately, and they are very strong and last long. This action of red blood may be easily understood: it increases the irritability of the muscular layer of the bowels, as it does for that of the muscles of animal life, and when it has been changed into black blood, it excites the muscular tissue and produces contraction. The strength and the long duration of the contraction in this case depend on the increase of irritability. When, as in the above experiment, black blood (containing a great quantity of carbonic acid, on account of the constant formation of that gas in blood deprived of the contact of atmospheric air) is injected, the irritability is not sensibly increased, but the excitation is considerable and there is an almost immediate effect.

If air is injected in the arteries of the bowels, soon after the death of the animal, a part of the blood it contains is expelled,

and we find that the movements do not last as long as if the blood had not been removed.

When an animal is killed by hæmorrhage, the intestine, as well as all the other organs, contains more blood than usual, and then its movements are not so strong, and last less than they do generally.

When in a recently asphyxiated animal the arteries and veins of a part of the bowels are divided, the movements of that part become less strong and last less than those of the other parts of the intestine.

When the bowels of a recently asphyxiated animal are put under a receiver containing carbonic acid, their movements are very much increased, but they do not last so long as when they are in the atmosphere.

When they are put under a receiver containing hydrogen, their movements are very quickly diminished in strength, and they last still less than when exposed to carbonic acid.

When they are put in oxygen, their movements diminish a little at first and soon after become stronger, and they last much longer than usual.

As a general conclusion about the apparently spontaneous contractions which I have described as taking place in paralyzed muscles during life or after death, I will say that it seems that black blood by its carbonic acid is the cause of these contractions. When the nervous centres are still united with the contractile tissues, we see, during agony or after death, stronger movements generally than when they are separated. The action of black blood on the nervous centres may be very great. I have found that the spinal cord, when separated from the encephalon, may be strongly excited by black blood. If an animal is asphyxiated after a transversal and complete division of its spinal marrow in the dorsal region, we see convulsions taking place in the posterior limbs, and they are nearly as strong as when the nervous centers have not been injured. The excitation on the spinal marrow is considerable enough to produce an erection of the penis.*

* Almost all, if not all, the secretions of the body are increased during asphyxia: bile, (as shown by Professor Bouisson,) saliva, tears, gastric, pancreatic and intestinal juices, and also liver-sugar, etc., are produced in

XXXIV.—ON THE CAUSE OF THE BEATINGS OF THE HEART.

The cause of the rhythmical movements of the heart has been heretofore unknown. I believe I have discovered it.

Before exposing my theory and the facts upon which it is grounded, I will show that the theories put forward until now are not correct.

There are three theories only which are worthy of examination: 1st, that of Haller; 2d, that of Carpenter; 3d, that of Budge, Schiff, and others.

Haller has been very near the truth in admitting that the beatings of the heart were excited by the blood. His error has been an *error loci*. He thought that the blood acted in the cavities of the heart. It is not so; and it is known that the heart may continue to beat after all the blood has been drawn out of its cavities.

The doctrine of Carpenter* is a very simple and remarkable one. He believes that the muscular fibres may act without having been excited. A muscle, says he, may be compared to the electric jar, and become so *charged* with *motility*, (or motor force,) as to execute spontaneous contractions; and elsewhere, "It is not very difficult to conceive that the ordinary rhythmical movements of the heart may be due to a simple excess of this motility, which is continually being supplied by the nutritive operations, and is as constantly discharging itself in contractile action." Carpenter believes that the reason for which the heart presents spontaneous contractions while the other muscles do not, (at least ordinarily,) is, that there is a higher degree of motility in the heart. He considers as very important the facts I have discovered, that many other muscles besides the heart may present rhythmical movements. He thinks that these facts show there is a tendency to rhythmical movements in the muscles themselves, altogether

greater quantity than usual. I believe that this increase results from the excitation of the nervous system, and, in some measure, perhaps, from a direct action of black blood on the capillaries of the glands. The urinary secretion may also be changed in asphyxia, and not only then the urine may contain sugar, as Alvaro Reynoso has found, but also albumen.

*See his *Principles of Human Physiology*, American edition, by F. G. Smith. Philadelphia, 1853. pp. 130 to 132, 319, 325, and 471-72.

independent of the excitement to action which they receive through the nervous system.

The best ground for the hypothesis of Carpenter is that, according to him, the heart continues to beat, although it is not exposed to any excitation in certain circumstances. He says: "When every source of excitement is excluded, we cannot but perceive that these actions take place with a *spontaneity* which can scarcely be accounted for in any other way than by considering them as expressions of the vital activity of the component cells of these forms of muscular tissue, which manifests itself in this mode, when the developmental life of the cell has attained its maturity. And this view is strikingly confirmed by what we know of the origin and termination of these movements. For the action of the heart commences when, as yet, its contractile parietes consist but of an assemblage of ordinary-looking cells, no proper muscular tissue being evolved, and no nervous system being yet developed, from which the stimulus to the movement can proceed; and *it is impossible to assign any other cause for the movement under such circumstances*, than the attributes inherent in the tissues which perform it."

The first thing to be said against the view of Carpenter is, that his hypothesis is not necessary; because *it is possible to assign another cause for the movement of the heart under the circumstances* he speaks of. This will be proved hereafter.

The doctrine of Carpenter implies, that the degree of irritability (motility, motor force, contractility,—never mind the name) is greater in the heart than in the other muscles which have no spontaneous action. This is not the case. The degree of irritability, as judged by its duration after death, is generally greater in the muscle of animal life, than in the heart. The accepted sentence of Haller, *Cor ultimum moriens*, generally, is not true.

If Carpenter was right, we should see, during life, the apparently spontaneous contractions which take place in all the contractile tissues after death; because their irritability is at a higher degree in the first, than in the second case. Besides, we should not see oxygen, or red blood, diminish the frequency of the beatings of the heart; and black blood, or carbonic acid, increase that frequency.

An experiment, consisting in the research of the influence of *vacuo* on the heart, has been made by Tiedemann and by Dr. S. W. Mitchell, and Dr. T. H. Bache, (see Dunglison's *Physiol.*, vol. ii. p. 150.) It seems to me that the result of this experiment is in complete opposition to the doctrine of Carpenter. These experimenters have found that the beatings of a heart were speedily brought to a stand by the exhaustion of the air, and that they were renewed when it was re-admitted. If the view of the eminent British physiologist was right, we ought to see the heart continue to beat in *vacuo* about the same length of time as it would in hydrogen or nitrogen, because its irritability cannot be suddenly diminished enough by the exhaustion of the air. In these gases the heart of a mammal may beat for five or ten minutes or more, and the right auricle may beat for hours; and the heart of a frog may beat for one day. It is much more to account for the stopping of the heart's action in admitting that the excitant of that action is removed during the exhaustion of the air. John Reid had found that the heart of a frog had continued to beat *in vacuo*, but how long he does not say.*

I will relate hereafter many experiments of mine which are in opposition to the theory of Carpenter.

It is one of the most important questions in physiology, whether the nervous centres, the nerves, and the contractile tissues are able to act without stimulation. This question has not been yet entirely treated by any physiologist. I propose publishing a special paper on the subject. I will merely say here that there may be apparently spontaneous actions in the spinal cord, as well as in the muscles. For instance, very frequently, in a frog, after the removal of the brain and the medulla oblongata, we may see strong movements apparently spontaneous, but when we know that the slightest excitation of the skin, or of any other very sen-

* Art. Heart, in Todd's *Cyclop.*, vol. ii. p. 611. J. Reid says in the same page, "We ought to be more cautious in admitting the existence of this innate moving power, since it is in opposition to a well known law in the animal economy, that though the various tissues of an organised body are endowed with certain vital properties, yet the application of certain external and internal stimuli is necessary to produce their manifestations of activity. In fact it is from the action and reaction of these tissues and excitants upon each other that the phenomena of life result."

sitive part, may excite the spinal cord, and produce a reflex action, we are authorised to consider all the movements taking place as reflex actions. An excitation may have come to the spinal marrow from the bladder, from the bowels, from the lungs, (in which worms are almost always found in the cold seasons, i. e. at the time these phenomena are generally observed,) etc.

As to the spontaneity of action in muscles, I have tried to prove in a preceding article that it is a mere and false appearance.* I will prove hereafter that the cause of the apparently spontaneous contractions of the heart, is the same as that of the like contractions in other contractile tissues.

The physiologists who maintain that the beatings of the heart

* Carpenter says that the action of the uterus, as it shows itself, "not merely in the final parturient effort, but in local contractions that frequently occur during the latter months of gestation, (simulating the movements of the fetus,) are more satisfactorily accounted for by considering them as a discharge of accumulated power, than in any other mode." I will try to prove elsewhere that for the uterus, as well as for any other contractile tissue, there is no spontaneous action. The uterus, in pregnancy, becomes more and more irritable every day, and when its irritability has arrived at a very high degree, then the slight excitation produced by the carbonic acid normally contained in the blood is sufficient to put it in action. When the contractions have begun, they are very much increased by a reflex action. Every contraction is accompanied by a galvanic discharge on the nerves in the neighborhood of the muscular fibres which contract, and the sensitive nerves being thus excited, it results, 1st, that a pain is felt, the degree of which is in proportion to the degree of any contraction, and therefore with the degree of galvanic discharge;† 2d, that the spinal marrow is excited, and produces reflex movements in the uterus. Now, the more these reflex contractions are energetic, the more they are induced to take place again, on account of the galvanic discharge which accompanies them. So that there would be a constant increase in the intensity of the contractions if there were not four limits to them. 1st, there is no galvanic discharge when the muscular fibres *are contracted*; it is only at the time they *are contracting* that this discharge takes place; 2d, the primitive cause of contraction, the excitation of the muscular tissue, by carbonic acid, diminishes much during the contraction, because the caliber of the small blood-vessels is much diminished, and the blood expelled from them; 3d, every contraction of the uterus diminishes the degree of its irritability; 4th, the reflex power of the spinal cord becomes exhausted, or at least diminished.

† See, on this subject, my paper in the *Comptes rendus de la Société de Bologne*, en. 1850, t. ii. p. 172.

depend on the nervous system, appear to me to be greatly mistaken. They make a confusion between two things, greatly distinct, one from the other: they conclude from the fact that the nervous system is able to act on the heart, that its influence is necessary. It is the same kind of mistake which is so frequently made as to the influence of the nervous system on nutrition, on secretions, and on animal heat; because that system is able to act upon these functions, it is concluded that its influence is necessary.

The first argument to be adduced against the writers who admit, as necessary, the influence of the nervous system on the heart, is, that they change only the ground of the difficulty in doing so. Instead of having to explain why the heart acts rhythmically, they have to explain why the nervous system acts rhythmically on the heart. Not only they have not explained this rhythmic action of the nervous system, but, as far as I know, they appear not to have been aware that this was to be explained.

The second reason I will mention, is the fact, so well established by my friend Professor Lebert, that, in embryos, the heart beats when it is merely composed of cells, and when the nervous system has not yet appeared.

A third reason is, that, either in monsters, or in animals operated on by physiologists, there has been a long persistence of the beatings of the heart when a part of the cerebro-spinal centre did not exist, or had been removed. Any part may be in that case, even the medulla oblongata, as I have discovered. (See Art. xvi. p. 40.)

In opposition to the idea that the beatings of the heart depend on the microscopical ganglia existing in that organ, I will say, that, besides the fact that the heart beats in embryos before the nervous system exists, and besides the improbability that such a small amount of nervous matter should have so great a power, there are two good reasons against this strange theory:—
1. There have been found no ganglia, large or microscopical, in the auricles, in the sinuses of the pulmonary veins, or in those veins. All these parts, nevertheless, may continue to beat a long while, (even for hours,) after they have been separated from the ventricles where are the microscopical ganglia. 2. Rhythmical movements may exist in a great many other muscular parts of the

body, where there is no microscopical ganglion, and where these parts have ceased to be under the influence of the nervous centres.

The three theories which I have examined being unable to explain the beatings of the heart, I will now expose my theory, and discuss the three following questions:—1. What is the excitant which puts the heart in action? 2. Does that excitant act rhythmically? 3. Does that excitant act together directly on the muscular fibres of the heart, and on the nervous system; or does it act only on the muscular fibres?

After having solved these three questions, I will examine the objections which might be made to the doctrine I propose.

1. *What is the excitant which puts the heart in action?*

I believe that the beatings of the heart are excited by a principle existing in the blood, and that carbonic acid is that principle. This view is grounded on the following facts:

a. When we prevent a warm-blooded animal from breathing, the beatings of the heart become more frequent than before, for about one or two minutes. It is not on account of the emotion alone that it is so, because the same effect is produced when we asphyxiate suddenly an animal which has entirely lost his power of having emotions, in consequence of the action of chloroform.

b. Many times I have found, on myself and on one of my friends, that the beatings of the heart are rendered more active during asphyxia. We hold our breath for about three quarters of a minute, and during the last fifteen seconds the heart beats from two to four (in one case five) minutes more than when the respiration was free. We have made the experiment in the sitting position, avoiding any movement of the body in all the cases.

c. John Reid has discovered that when any hemadynamometer is put in the femoral artery of a dog, the mercury rises in the instrument if the animal is asphyxiated, and about one minute after the respiration has been stopped. The same result has been obtained in twenty experiments. It seems to me that this fact proves that the contractions of the heart become more energetic during asphyxia. John Reid attributes the result he has obtained to some difficulty that black blood seems to have in passing through the capillaries of the different parts of the body. I do not deny that there is such a difficulty; but I think that the great reason

of the ascension of mercury in the hemadynamometer is, the increase in the force of the heart. A simple experiment proves that I am right. I adapt the hemadynamometer to the aorta in the abdominal cavity, and then I open quickly the chest, and I put a ligature to the brachial and carotid arteries. About three quarters of a minute after opening the chest, and about half a minute after the ligature has been put on the arteries of the head and arms, the mercury rises notably in the instrument; sometimes the elevation is as considerable as two inches. It results from this experiment, that the heart beats more strongly in asphyxia about one minute after its beginning.

d. Woodall, a most intelligent and accurate observer, says Dr. Martin Paine, (see *Med. and Physiol. Comment.*, t. ii. p. 49,) states, that the best remedy for syncope is to obstruct respiration entirely by momentarily confining the nose and mouth. If this be true, it is in perfect accordance with my view, that, during asphyxia, the normal cause of the beating of the heart increases in the blood.

e. If a frog is put under a receiver containing pure oxygen, at a temperature of 40 or 50° Fahr. (4, 5, or 10 Cent.) after its heart has been laid bare and its central nervous system destroyed, we see the heart beat for a very long time, (one, two, or three days.) On the contrary, if, at the same temperature, another frog, deprived also of the central nervous system, is put in carbonic acid gas, the heart beats very quickly at first, but it soon ceases to beat, (in one or two hours only, sometimes, and for the most about half a day.)

f. All the causes which increase the formation of carbonic acid gas in the body, increases the frequency of beatings of the heart.

g. If we inject the serum of blood into the arteries of the heart, so as to expel as completely as possible the blood contained in the capillaries of this organ, and if then we remove the blood from the cavities of the heart, we find that its beatings are, at once, almost entirely suspended, and that they are completely stopped in a very short time, (from one to eight minutes.) The muscular irritability is not destroyed in this organ; it does not beat because its excitant has been removed.

h. I have found that when the heart of a young animal is put in hydrogen, its beatings hardly change at first, but they stop in

a very short time. When it is put in carbonic acid gas, its beatings are, at first, increased in frequency and strength; but they very soon are stopped. When it is put in oxygen, its beatings are slowly increased in frequency and strength, and they last very long.

i. On newly-born cats and dogs, before the occlusion of the *ductus arteriosus*, I open the chest and put a ligature on the arteries going to the head and fore limbs, and on the aorta immediately after the origin of the *ductus arteriosus*. Then the blood, expelled from the right ventricle, is sent to the lungs, from which it comes to the left auricle, and afterwards to the left ventricle. From there it is sent into the only part of the aorta remaining accessible, and thence it goes into the cardiac arteries, and into the pulmonary artery, through the *ductus arteriosus*, (a direction which is the reverse of the normal direction in that duct.) By the cardiac veins the blood arrives again in the right side of the heart. The circulation from the heart to the lungs, and *vice versa*, continues very well. I have found, that if hydrogen is insufflated into the lungs, the beatings of the heart are not much changed at first, but they go on diminishing, and they disappear in a short time. When an injection is made with carbonic acid, the beatings of the heart are quickly increased in frequency and strength; but they are stopped after a short time. When oxygen is insufflated, the beatings of the heart become slowly more frequent, and they remain quick and strong for a long time. (I have once, by such insufflation of oxygen, maintained beating for eleven hours in the heart of a young cat.)

I believe that these facts prove that black blood, by its carbonic acid, is an excitant of the beatings of the heart. If, now, we adduce to these facts all those I have related in a preceding article, on the apparently spontaneous contractions in all the contractile tissues of the body, we shall have a very considerable number of facts, proving that, during asphyxia, there is an accumulation in the blood of the principle which causes these contractions. I believe that it is almost impossible to deny that this principle is the carbonic acid gas.

Before trying to show that what takes place in asphyxia in the heart is only an exaggeration of what normally exists in that organ, I will treat the two remaining of the three questions I

have announced I would endeavor to solve, as regards the excitant of the heart's action.

2. *Why does that excitant act rhythmically?*

I believe it is easy to explain why the agent of excitation of the heart* produces rhythmical contractions. I will suppose, first, that the action is permanent. A part of the heart, ventricles, or auricles, being dilated, receives an excitation in all its fibres simultaneously, and a contraction is produced. But, according to the well-known law of Schwann, the exciting cause which is able to give the impulse when the muscular fibres are long, is not able to maintain the contraction when the fibres have been shortened. Then, on account of this insufficiency of power of the cause of the contraction, a dilatation ensues. We may present the fact in other words, and say that the resistance to the contraction originating from the displacement of the constitutive matter of the contractile tissues, increases in proportion to the shortening of the fibres; and that after the fibres have contracted under the impulse of the exciting cause, although this cause continues to act, a dilatation is produced by the force belonging to that resistance, which is nothing but elasticity. If the cause of the contraction of the heart was a considerable one, then we should see a permanent contraction; and it is so when we apply galvanism—the elasticity, then, is not powerful enough to produce dilatation. On the contrary, with a weak exciting cause, like carbonic acid, the result ought to be different. When that cause has more power, as in asphyxia, the shortening of the fibres takes place quicker, and is more considerable; and even then it is not sufficient to maintain contraction, the tendency to dilatation being also increased.

I ought to say, that the excitant cause of the contractions is not always at the same degree of power. The small blood-vessels and the capillaries being compressed during the muscular contractions, there is a diminution of excitation during that time. This should be sufficient to explain the alternate contractions and dilatations. But such a diminution in the caliber ought to be very little, if even it exists in certain organs, (the heart when composed of cells, for instance.)

* What I will say here for the heart, might be said for all the contractile tissues, presenting apparently spontaneous rhythmical contractions, as the cilia, for instance.

I come now to the third question about the excitant of the heart—

3. *Does that excitant act together on the muscular fibres and on the nerves of the heart, or does it act only on the muscular fibres?*

I believe it ought to act also on the nerves; but I cannot prove it otherwise than by saying, that all the agents of excitation that we know to act on the muscular fibres, are able to act on the nerves.

There are many things to be said besides the above facts and reasoning, to prove the truth of the doctrine I propose. I will expose some of them.

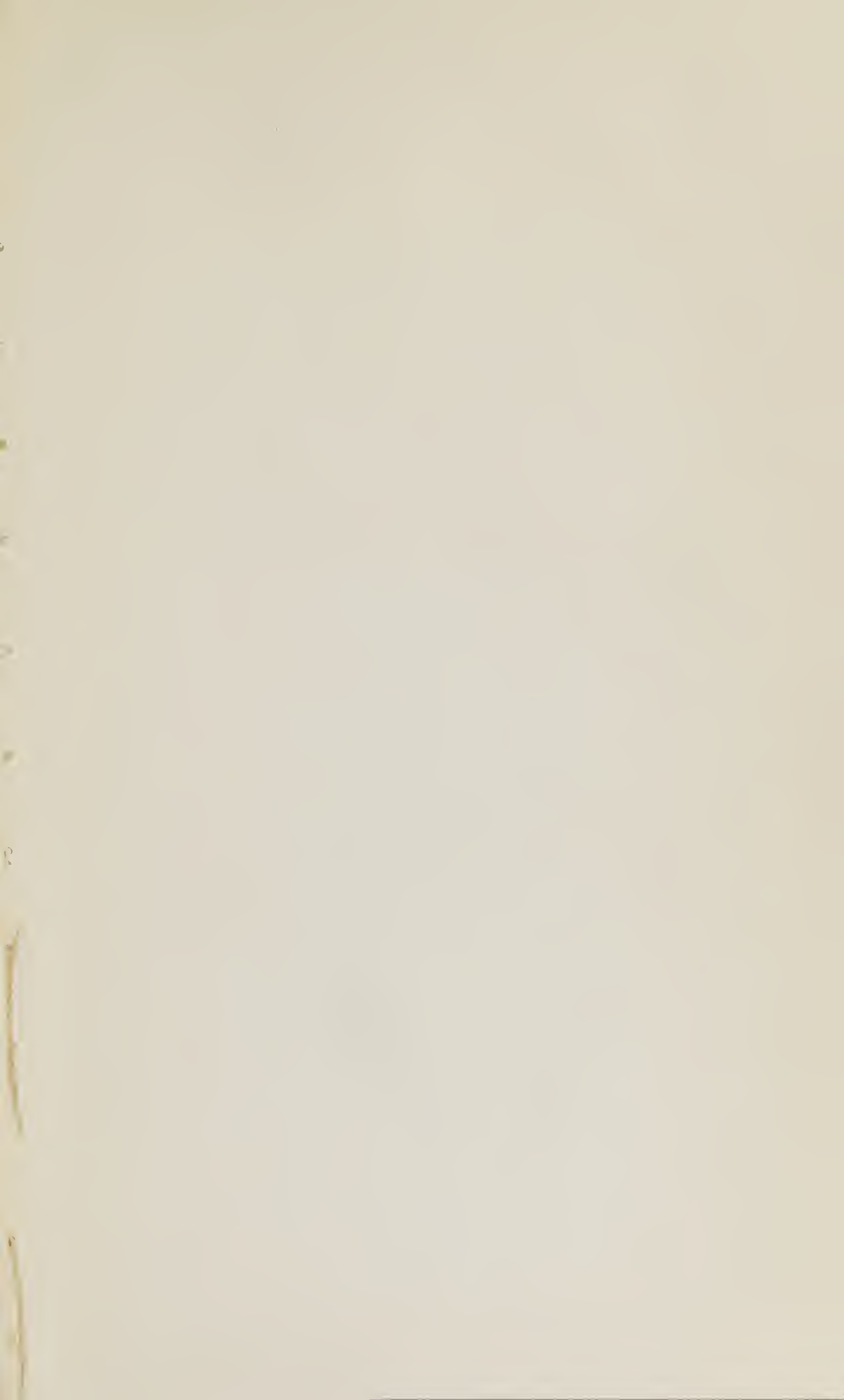
The following question might be made:

How is it that the heart is the only muscle containing striated fibres, which presents normally rhythmical movements?

The answer to this question appears to be very simple. The intensity of the stimuli, the degree of irritability, and the resistance which a muscle has to overcome when it contracts, are three elements which we ought not to lose sight of when we examine the difference of contractions between two muscles. Suppose the heart possessing the same degree of irritability as another muscle: if the stimulus is the same, and the resistance the same also, for the heart and for the other muscle, there will be the same effects. But if the stimulus is more considerable in the heart than in the other muscle, and if the resistance to be overcome is less for the heart, then with the same degree of irritability in both parts, and even with less irritability in the heart than in the other muscle, we will see a movement in the heart, and not in that other muscle. Now a simple examination of the vessels of the heart, proves that they contain more blood, and consequently more stimulus, than the other striated muscles. Besides, as the heart is not inserted into heavy bones to be moved, it has less resistance to overcome when it has not to circulate the blood, as after death, or when it is out of the chest, than the muscles of animal life. Some muscles in the face and the diaphragm, being almost without an external resistance, when their contractions do not go so far, it results that they are moved much more easily after death, than the muscles of the limbs. In consequence of these views, I believe that, although there is in the blood-vessels

of all the muscles of the body a principle which is an exciting cause of contractions, there are no contractions produced, because the quantity of that principle is not sufficient, or because the resistance to contractions in many muscles is greater than in the heart.

I must, in conclusion, say, that I do not advance my theory of the rhythmical movements as perfectly proved. I believe it is true, and that there are a great many facts which appear positively to prove it. What I can assert is, that it is by far much more in harmony with all the known facts, than the other theories.





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